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TRANSACTIONS

OF THE

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SURGICAL ASSOCIATION.

VOLUME THE FIRST.

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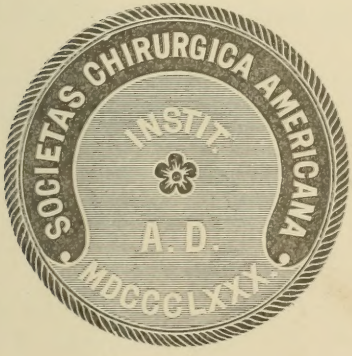

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FELLOWS

OF THE

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JUNE, 1883.

* Denotes Original Members.
† Denotes Fellows who have resigned.
‡ Denotes Fellows who are deceased.

ELECTED

1882. AGNEW, D. HAYES, M.D., LL.D., Professor of Surgery and of Clinical Surgery in the University of Pennsylvania, Surgeon to the Pennsylvania Hospital, to the University Hospital, and Consulting Surgeon to the Orthopædic Hospital. Philadelphia.

*1880. ASHHURST, JOHN, Jr., M.D., Professor of Clinical Surgery in the University of Pennsylvania, Senior Surgeon to the Children's Hospital of Philadelphia, etc. Philadelphia.

1880. BARTON, J. M., M.D. Philadelphia.

*1880. BENHAM, SILAS NELSON, A.M., M.D. Pittsburgh, Pa.

1882. BILLINGS, JOHN SHAW, M.D., Surgeon U. S. Army. Washington, D. C.

1882. BONTECOU, REED BROCKWAY, M.D., Surgeon to Marshall Infirmary of Troy, N. Y. Troy, N. Y.

1882. BOZEMAN, NATHAN, M.D., Surgeon to the Woman's Hospital of the State of New York. New York.

*1880. BRAMBLE, D. D., M.D. Cincinnati.

*1880. BRIGGS, WILLIAM THOMPSON, M.D., Professor of Surgery in the Medical Department of the University of Nashville and Vanderbilt University, Surgeon to the University Hospital of Nashville. Nashville, Tenn.

ELECTED

*1880. Brinton, John H., M.D., Professor of the Practice of Surgery and of Clinical Surgery in the Jefferson Medical College of Philadelphia, Surgeon to St. Joseph's Hospital of Philadelphia. Philadelphia.

‡*1880. Brock, Hugh W., M.D. Morgantown, West Virginia.

*1880. Byrd, William Andrew, M.D., Professor of Clinical Surgery in the Quincy College of Medicine, Consulting Surgeon to the Sarah Denman Hospital. Quincy, Ill.

1882. Bull, William Tillinghast, M.D., Surgeon to the New York Hospital, to St. Luke's Hospital, and Consulting Surgeon to the Hospital for Ruptured and Crippled. New York.

*1880. Cabell, James L., A.M., M.D., LL.D., Professor of Surgery in the University of Virginia. University of Virginia.

*1880. Campbell, Henry Fraser, A.M., M.D., Professor of Surgery and Gynæcology in the Medical Department of the University of Georgia, Surgeon to the City Hospital and to the Freedman's Hospital of Augusta, Ga. Augusta, Ga.

1882. Cheever, David Williams, M.D., Professor of Surgery in the Harvard University, Surgeon to the Boston City Hospital. Boston.

*1880. Coleman, John Scott, M.D. Augusta, Ga.

1881. Cole, Richard Beverly, M.D., Ph.D., Professor of Gynæcology in the Medical Department of the University of California. San Francisco.

1882. Comingor, John A., M.D., Professor of Surgery in the Indiana Medical College, Surgeon to the City Hospital of Indianapolis. Indianapolis, Indiana.

*1880. Conner, Phineas Sanborn, M.D., Professor of Anatomy and of Clinical Surgery in the Medical College of Ohio, Professor of Surgery in the Medical Department of Dartmouth College, Hanover, New Hampshire, Surgeon to the Cincinnati Hospital and to the Good Samaritan Hospital. Cincinnati.

1882. Cunningham, Francis Deane, M.D. Richmond, Va.

ELECTED

1883. Dandridge, N. P., M.D. Cincinnati.

1882. Davis, J. S., M.D. University of Virginia.

*1880. Davis, Samuel T., M.D. Lancaster, Pa.

*1880. Dawson, William Wirt, M.D., Professor of Surgery in the Medical College of Ohio, Surgeon to the Hospital of the Good Samaritan. Cincinnati.

1882. Dennis, Frederick Shepard, M.D., Professor of the Principles and Practice of Surgery in the Bellevue Hospital Medical College, Surgeon to the Bellevue Hospital, to St. Vincent's Hospital, and Consulting Surgeon to the Hospital for Ruptured and Crippled. New York.

1882. Detmold, William, M.D., Professor of Military and Clinical Surgery in the College of Physicians and Surgeons of Columbia College (Emeritus), Consulting Surgeon to the Presbyterian Hospital of New York. New York.

*1880. Dugas, Louis Alexander, M.D., LL.D., Professor of Surgery in the Medical College of Georgia (Emeritus), Surgeon to the City Hospital. Augusta, Ga.

1882. Dunott, Thomas J., M.D., Surgeon to the Harrisburg Hospital. Harrisburg, Pennsylvania.

1883. Fenger, Christian, M.D. Chicago.

1882. Fifield, William Cranch Bond, M.D., M.R.C.S. Lond., Surgeon to the Boston City Hospital. Boston.

1882. Forbes, William Smith, M.D., Demonstrator of Anatomy in the Jefferson Medical College of Philadelphia, Surgeon to the Protestant Episcopal Hospital of Philadelphia. Philadelphia.

1882. Gay, George Washington, M.D., Surgeon to the Boston City Hospital. Boston.

*1880. Gouley, J. W. S., M.D. New York.

‡*1880. Greene, W. Warren, M.D. Portland, Maine.

1882. Gregory, Elisha H., M.D., Professor of Surgery in the St. Louis Medical College, Surgeon to the St. Louis Hospital. St. Louis.

ELECTED

*1880. Gross, Samuel D., M.D., LL.D., D.C.L. Oxon., LL.D. Cantab., Professor of Surgery in the Jefferson Medical College of Philadelphia (Emeritus). Philadelphia.

*1880. Gross, Samuel W., A.M., M.D., Professor of the Principles of Surgery and of Clinical Surgery in the Jefferson Medical College of Philadelphia. Philadelphia.

*1880. Gunn, Moses, A.M., M.D., LL.D., Professor of Surgery in the Rush Medical College, Surgeon to St. Joseph's Hospital. Chicago.

1882. Hewson, Addinell, M.D. Philadelphia.

‡*1880. Hodge, H. Lenox, M.D., Demonstrator of Anatomy in the University of Pennsylvania, Surgeon to the Presbyterian Hospital. Philadelphia.

‡*1880. Hodgen, John T., M.D., Professor of Anatomy and of Clinical Surgery in the St. Louis Medical College. St. Louis.

‡*1880. Hughes, J. C., M.D. Keokuk, Iowa.

*1880. Hutchison, Joseph Chrisman, M.D., LL.D., Surgeon to the Brooklyn City Hospital and to the Brooklyn Orthopædic Infirmary, Consulting Surgeon to the Kings County Hospital, to St. John's Hospital, and to St. Peter's Hospital. Brooklyn.

1882. Isham, R. N., M.D. Chicago.

*1880. Johnston, Christopher, M.D., Professor of Surgery in the University of Maryland (Emeritus). Baltimore.

*1880. Keller, James McDonald, M.D. Hot Springs, Arkansas.

*1880. Keen, William Williams, A.M., M.D., Professor of the Principles and Practice of Surgery in the Woman's Medical College of Philadelphia, Surgeon to St. Mary's Hospital of Philadelphia. Philadelphia.

*1880. Kinloch, Robert Alexander, M.D., Professor of Surgery in the Medical College of the State of South Carolina, Surgeon to the Roper Hospital, to the City Hospital, and Surgeon and Gynæcologist to St. Xavier's Infirmary. Charleston.

ELECTED

*1880. LEVIS, RICHARD J., A.M., M.D., Surgeon to the Pennsylvania Hospital, and to the Jefferson College Hospital of Philadelphia. Philadelphia.

1882. LITTLE, J. L., M.D. New York.

1883. MARKOE, THOMAS MASTERS, M.D., Professor of the Principles of Surgery in the College of Physicians and Surgeons. New York.

*1880. MARKS, SOLON, M.D., Surgeon to St. Mary's Hospital of Milwaukee. Milwaukee, Wisconsin.

*1880. MASTIN, CLAUDIUS HENRY, M.D., LL.D., Surgeon to the Providence Infirmary. Mobile, Alabama.

1883. MCCANN, JAMES, M.D., Surgeon to the Western Pennsylvania Hospital. Pittsburgh, Pennsylvania.

1882. MCGRAW, THEODORE A., M.D., Professor of Surgery in the Detroit Medical College, Surgeon to St. Mary's Hospital, and to the Harper Hospital. Detroit, Michigan.

*1880. MCGUIRE, HUNTER, M.D., Senior Surgeon to St. Luke's Hospital of Richmond. Richmond, Va.

1882. MACLEAN, DONALD, M.D., Professor of Surgery and of Clinical Surgery in the University of Michigan, Surgeon to the University Hospital of Ann Arbor. Detroit, Michigan.

1882. MCLEAN, LEROY, M.D., Surgeon to the Troy Hospital, in charge of the Sisters of Charity. Troy, N. Y.

*1880. MEARS, J. EWING, A.M., M.D., Professor of Anatomy and of Clinical Surgery in the Pennsylvania College of Dental Surgery, Demonstrator of Surgery in the Jefferson Medical College of Philadelphia, Surgeon to St. Mary's Hospital, and Gynæcologist to the Jefferson Medical College Hospital of Philadelphia. Philadelphia.

*1880. MOORE, EDWARD MOTT, M.D., LL.D., Surgeon to St. Mary's Hospital of Rochester. Rochester, N. Y.

*1880. MORTON, THOMAS GEORGE, M.D., Professor of Clinical and of Operative Surgery in the Philadelphia Polyclinic and College for Graduates, Surgeon to the Pennsylvania Hospital, to the Orthopædic Hospital, and to the Jewish Hospital. Philadelphia.

ELECTED

1882. Nancrede, Charles Beylard, M.D., Surgeon to the Protestant Episcopal Hospital and to St. Christopher's Hospital for Children. Philadelphia.

1882. Norris, Basil, M.D., U. S. Army, Lieutenant-Colonel and Surgeon U. S. Army. Washington, D. C.

1882. Owens, John E., M.D., Professor of Surgical Anatomy and Operations of Surgery in the Chicago Medical College, Attending Surgeon to St. Luke's Hospital of Chicago. Chicago.

*1880. Packard, John H., M.D., Surgeon to the Episcopal Hospital and to St. Joseph's Hospital. Philadelphia.

*1880. Pancoast, William Henry, M.D., Professor of Anatomy in the Jefferson Medical College of Philadelphia, Surgeon to the Philadelphia Hospital. Philadelphia.

1882. Parkes, Charles T., M.D. Chicago.

1883. Peck, Washington F., A.M., M.D., Professor of Surgery and of Clinical Surgery in the Medical Department of the State University of Iowa, Surgeon to the Mercy Hospital of Davenport, and Consulting Surgeon to the Mercy Hospital of Iowa City. Davenport, Iowa.

†1882. Peters, George A., M.D. New York.

*1880. Pollock, Alex. M., M.D., Surgeon to the Mercy Hospital and to the Passavant's Hospital of Pittsburgh. Pittsburgh, Pa.

1882. Porter, William Gibbs, M.D., Surgeon to the Presbyterian Hospital and to the Philadelphia Hospital. Philadelphia.

1882. Prewitt, Theodore F., M.D., Professor of the Principles and Practice of Surgery and of Clinical Surgery in the Missouri Medical College, Surgeon to St. John's Hospital. St. Louis.

1882. Prince, David, M.D. Jacksonville, Illinois.

1882. Reed, Thomas B., M.D., Surgeon to the Presbyterian Hospital. Philadelphia.

ELECTED

1882. ROBERTS, JOHN B., M.D., Professor of Applied Anatomy and of Operative Surgery in the Philadelphia Polyclinic and College for Graduates in Medicine, Surgeon to St. Mary's Hospital of Philadelphia. Philadelphia.

*1880. RICHARDSON, TOBIAS G., M.D., Professor of Surgery in the Medical Department of the University of Louisiana, Surgeon to the Charity Hospital. New Orleans.

1882. RUSHMORE, JOHN DIKEMAN, M.D., Professor of Surgery in the Long Island College Hospital of Brooklyn, Surgeon to the Brooklyn Hospital, to St. Peter's Hospital, and to the Brooklyn Eye and Ear Hospital. Brooklyn, N. Y.

*1880. RUSSELL, THOMAS PEMBER, M.D. Oshkosh, Wisconsin.

*1880. SAYRE, LEWIS ALBERT, M.D., Professor of Orthopædic Surgery in the Bellevue Hospital Medical College, Consulting Surgeon to the Bellevue Hospital, to the Charity Hospital, to St. Elizabeth's Hospital, and to the Home for Incurables. New York.

1882. SENN, NICHOLAS, M.D., Surgeon to the Milwaukee Hospital. Milwaukee, Wisconsin.

1882. SMITH, ALAN PENNIMAN, M.D., Surgeon to the St. Vincent's Hospital, to the Union Protestant Infirmary, to the Hebrew Hospital, to St. Agnes's Hospital, to the Maryland Eye, Ear, and Throat Hospital, and to the Hospital for Diseases of Women. Baltimore, Md.

1882. SMITH, STEPHEN, A.M., M.D., Professor of Clinical Surgery in the University Medical College of New York, Surgeon to the Bellevue Hospital and to St. Vincent's Hospital. New York.

1882. SABINE, THOMAS TAUNTON, M.D., Professor of Anatomy in the College of Physicians and Surgeons, New York. New York.

1882. SQUIRE, T. H., M.D. Elmira, N. Y.

*1880. SWINBURNE, JOHN, M.D. New York.

ELECTED

1883. Taylor, William Edwin, M.D., Professor of Principles and Practice of Surgery in the Medical Department of the University of California, Professor of Principles and Practice of Surgery in the Dental Department of the University of California. San Francisco.

1882. Thomson, William, M.D., Honorary Professor of Ophthalmology in the Jefferson Medical College of Philadelphia, Ophthalmic Surgeon to the Jefferson Medical College Hospital. Philadelphia.

1882. Thompson, J. W., M.D. Paducah, Ky.

1882. Tiffany, Louis McLane, M.D., B.A. Cantab., Professor of Surgery in the University of Maryland, Surgeon to the University Hospital. Baltimore.

*1880. Tremaine, William S., M.D., Major and Surgeon U. S. Army, Professor of Surgery in the Niagara University of Buffalo, N. Y., Surgeon to the Buffalo Hospital of Sisters of Charity. Buffalo, N. Y.

1882. Vanderveer, Albert, A.M., M.D., Ph.D., Professor of Principles and Practice of Surgery and of Clinical Surgery in the Albany Medical College, Consulting Surgeon to St. Peter's Hospital, and Attending Surgeon to the Albany Hospital, of Albany, N. Y. Albany, N. Y.

*1880. Wales, Philip Skinner, M.D., Medical Director and Chief of Bureau of Medicine and Surgery, U. S. Navy. Washington, D. C.

1882. Warren, John Collins, M.D., Assistant Professor of Surgery in the Harvard University, Surgeon to the Massachusetts General Hospital. Boston.

1882. Watson, B. A., A.M., M.D., Surgeon to the Jersey City Charity Hospital, to St. Francis's Hospital, and to Christ's Hospital. Jersey City, N. J.

*1880. Weist, Jacob Rowland, A.M., M.D. Richmond, Ind.

*1880. Westmoreland, W. F., M.D. Atlanta, Ga.

ELECTED

1882. WHITE, J. WILLIAM, M.D., PH.D., Demonstrator of Surgery, Lecturer on Operative and Minor Surgery, Lecturer on Venereal and Genito-Urinary Diseases in the University of Pennsylvania, Surgeon to the Philadelphia Hospital, and Assistant Surgeon to the University Hospital. Philadelphia.

1882. WILLARD, DEFOREST, M.D., PH.D., Lecturer on Orthopædic Surgery in the University of Pennsylvania. Philadelphia.

‡*1880. WOOD, JAS. R., M.D., Professor of the Principles and Practice of Surgery and of Clinical Surgery in the Bellevue Hospital Medical College, New York. New York.

*1880. YANDELL, DAVID W., M.D., Professor of the Principles of Surgery and of Clinical Surgery in the Medical Department of the University of Louisville. Louisville, Kentucky.

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1882. HUNT, WILLIAM, M.D., Surgeon to the Pennsylvania Hospital and to the Orthopædic Hospital. Philadelphia.

1882. PARKER, WILLARD, M.D., LL.D., Professor of Surgery in the College of Physicians and Surgeons of New York City (Emeritus), Consulting Surgeon to the Bellevue Hospital, to the New York Hospital, to St. Luke's Hospital, to the Roosevelt Hospital, and to the Mt. Sinai Hospital. New York.

1882. POST, ALFRED C., M.D. New York.

1882. SIMS, J. MARION, M.D. New York.

CONTENTS.

	PAGE
Officers of the Association	iii
List of the Presidents of the Association	iv
Fellows of the Association	v
Honorary Fellows	xiii
List of Illustrations	xix
Notice	xx
Address of Welcome	xxi
A Minute of Regret	xxvii
Address of Welcome	xxix
Valedictory Address	xxxi

The Influence of Operations upon the Prolongation of Life and Permanent Recovery in Carcinoma of the Breast. By SAMUEL W. GROSS, A.M., M.D., Surgeon to, and Lecturer on Clinical Surgery in, the Jefferson Medical College Hospital, and the Philadelphia Hospital . 1

Report of a Case of Supposed Spontaneous Aneurism of the Posterior Tibial Artery. Ligature of Femoral Artery; Incision into Sac one month after, followed by serious Hemorrhage; Amputation through the Thigh; Recovery; also, a *résumé* of the Literature on the subject. By R. A. KINLOCH, M.D., Professor of Surgery in the Medical College of the State of South Carolina, Charleston, S. C. 9

Report of some Surgical Cases. By JOHN H. PACKARD, M.D., Surgeon to the Episcopal Hospital, Philadelphia, Pa. 19

On Sanitary Conditions in relation to the Treatment of Surgical Operations and Injuries. By JAMES L. CABELL, A.M., M.D., LL.D., President National Board of Health; Ex-President American Public Health Association; Ex-President and Honorary Fellow Medical Society of Virginia; Professor of Surgery and Physiology in University of Virginia, etc. 23

Treatment of Fractures of the Skull, Recent and Chronic, with Depression. By MOSES GUNN, M.D., Professor of Surgery in the Rush Medical College, Chicago, Illinois 83

PAGE

The Treatment of Transverse Fracture of the Patella with the object of producing Bony Union. By R. J. Levis, M.D., Surgeon to the Pennsylvania Hospital and to the Jefferson Medical College Hospital, Philadelphia, Pa. 99

Section of the Femur, for Relief of Bony Anchylosis of the Hip-Joint in bad position, with Presentation of the Specimen. By E. M. Moore, M.D., of Rochester, New York 111

Foreign Bodies in the Air-Passages. A Study of 1000 Cases to determine the Propriety of Bronchotomy in such Accidents. By J. R. Weist, M.D., of Richmond, Indiana 117

The Antiseptic Treatment of Wounds after Operations and Injuries. By W. T. Briggs, M.D., Professor of Surgery in Medical Departments of University of Nashville and Vanderbilt University, Nashville, Tenn. 137

A Résumé of the Etiology, Pathology, Diagnosis, and Treatment of Morbus Coxarius. By Joseph C. Hutchison, M.D., LL.D., of Brooklyn, New York 159

A Case of Bony Union after Impacted Intra-Capsular Fracture of the Neck of the Femur. By N. Senn, M.D., of Milwaukee, Wisconsin . 167

Condition of the Cicatrices after Complete Extirpation of the Breast for Carcinoma. By Samuel W. Gross, M.D., Professor of the Principles of Surgery and Clinical Surgery in the Jefferson Medical College of Philadelphia 171

The Intra-Peritoneal Method of Treating the Pedicle in Ovariotomy, with a Report of some Experiments made. By J. Ewing Mears, M.D., Gynæcologist to Jefferson Medical College Hospital, Surgeon to St. Mary's Hospital, etc. 173

Have we any Therapeutic Means, as Proven by Experiment, which directly affect the Local Processes of Inflammation? By C. B. Nancrede, M.D., Surgeon to the Episcopal Hospital, Philadelphia, Pa. . 185

Lister's System of Aseptic Wound-Treatment *versus* its Modifications. By B. A. Watson, A.M., M.D., Surgeon to the Jersey City Charity and St. Francis Hospitals, Jersey City, New Jersey 205

Ligation of the Primitive Iliac Artery for the Relief of Secondary Hemorrhage after Amputation at the Hip-Joint. By John H. Packard, M.D., Surgeon to the Episcopal Hospital, Philadelphia, Pa. 225

The Use of the Trephine in Traumatic Empyema associated with Thoracic Fistula. By T. G. Richardson, M.D., Professor of Surgery in the Medical Department of the University of Louisiana, New Orleans 241

Value of Early Operations in Morbid Growths. By S. D. Gross, M.D., LL.D., D.C.L. Oxon., LL.D. Cantab. 253

Dislocations of the Astragalus. By Basil Norris, M.D., Surgeon U. S. A. 265

Excisions of the Tarsus, with a Report of Two Successful Removals of the Entire Tarsus. By P. S. Conner, M.D., Professor of Anatomy and Clinical Surgery in the Medical College of Ohio, etc. 285

PAGE

Trephining of the Sternum for Removal of a Foreign Body from the Anterior Mediastinum. With the Report of the Case. By S. Marks, M.D., of Milwaukee, Wisconsin 307

Fracture of the Neck of the Femur. By E. M. Moore, M.D., of Rochester, New York 313

Fractures of the Neck of the Femur, with Special Reference to Bony Union after Intra-Capsular Fracture. By N. Senn, M.D., Surgeon to the Milwaukee Hospital, Milwaukee, Wisconsin 333

A Peculiar Form of Traumatic Aneurism of the Femoral Artery. With Exhibition of the Specimen. By T. G. Richardson, M.D., Professor of Surgery in the Medical Department of the University of Louisiana, New Orleans 453

A Case of Nephrectomy for Medullary Carcinoma, and Partial Cholecystectomy for Calculus, in the same Subject. By Samuel W. Gross, A.M., M.D., Professor of the Principles of Surgery and Clinical Surgery in the Jefferson Medical College of Philadelphia . . . 461

Closure of the Jaws and its Treatment, with the Report of a Case in which complete occlusion followed a Gunshot Wound of the Left Superior Maxilla, received at two-and-a-half years of age, and which was relieved eighteen years subsequently by operation according to a new method. By J. Ewing Mears, M.D., Professor of Anatomy and Clinical Surgery in the Pennsylvania College of Dental Surgery, Demonstrator of Surgery in the Jefferson Medical College, etc. 469

Removal of Meckel's Ganglion for the Relief of Trifacial Neuralgia, with Report of Cases. By A. Vanderveer, M.D., Surgeon to the Albany Hospital, Albany, New York 489

Splint for the Treatment of Synovitis of the Wrist-Joint. By J. M. Barton, M.D., Surgeon to the German Hospital, Philadelphia . . 503

External Median Perineal Urethrotomy for Cystitis and for the Removal of Morbid Growths from the Bladder. By R. B. Bontecou, M.D., of Troy, New York 505

Description of a Rectal Obturator, and a Report upon its Uses. By David Prince, M.D., of Jacksonville, Illinois 509

Strictures of the Œsophagus. Their Nature and Treatment, with Cases. By Henry F. Campbell, M.D., of Augusta, Georgia 517

Report of Cases Illustrating the Conditions of Luxation of the Ulna in connection with Colles's Fracture. By E. M. Moore, M.D., of Rochester, New York 551

LIST OF ILLUSTRATIONS.

PAGE

Report of Some Surgical Cases. (John H. Packard, M.D.) . . . 20

Treatment of Morbus Coxarius. Figs. 1–3. (Joseph C. Hutchison, M.D.) 161–162

Treatment of the Pedicle in Ovariotomy. (J. Ewing Mears, M.D.) . 181

Ligation of the Primitive Iliac Artery. (John H. Packard, M.D.) . 230

Dislocations of the Astragalus. (Basil Norris, M.D.) 265

Fractures of the Neck of the Femur. Figs. 1–19. (N. Senn, M.D.) 341–436

Traumatic Aneurism of the Femoral Artery. (T. G. Richardson, M.D.) 457

Closure of the Jaws and its Treatment. Figs. 1 and 2. (J. Ewing Mears, M.D.) 480–481

Trifacial Neuralgia. Figs. 1 and 2. (A. Vanderveer, M.D.) . 491–492

Splint for the Treatment of Synovitis of the Wrist-Joint. (J. M. Barton, M.D.) 503

Rectal Obturator. (David Prince, M.D.) 509

Luxation of the Ulna in connection with Colles's Fracture. Figs. 1–8. (E. M. Moore, M.D.) 555–564

NOTICE.

This volume of Transactions contains the papers read before the Association from September, 1880, to June, 1883.

The Association assumes no responsibility for the statements and opinions published in this volume.

ADDRESS OF WELCOME

BY THE PRESIDENT,

S. D. GROSS, M.D., LL.D., D.C.L. OXON., LL.D. CANTAB.

[Delivered at the opening of the meeting, held in Philadelphia, May 31, 1882.]

GENTLEMEN OF THE AMERICAN SURGICAL ASSOCIATION:—

The Executive Committee in charge of this meeting, which I now formally open, have assigned to me the pleasing duty of congratulating you upon your arrival in our city, and of welcoming you to our homes and hearts. It would be needless to indulge in assurances of what we are going to do for you; but this I may say, that, as good and loyal brothers, we shall spare no efforts to make your time pass agreeably and profitably. A reunion like this may be viewed under two aspects, the practical and the social. Naturally the first object is the former, and the programme which has been placed in your hands is sufficient evidence that we mean work. The meetings heretofore, with the exception of the one held at Coney Island, last September, were all of a strictly executive character, intended simply to perfect the organization of the Association, and to put us in a better position for future usefulness. Hardly less important than this is the social feature of such a reunion. To cultivate kindly feeling, and to strengthen and cement the bonds of good fellowship, are objects of the highest consideration. If such a meeting were intended only for the discussion of scientific papers, and the presentation of cases of diseases and injuries, it would indeed be a cold and tame, if not heartless, affair.

The question has been asked, perhaps pertinently enough, what need is there of such an organization as this, seeing that the country is full of all sorts of medical societies? The answer is not difficult; at all events, I do not find it so. We have in the United States, according to a reasonable estimate, not fewer

than sixty thousand medical men. Among these are large numbers of surgeons, who, in point of culture, practical skill, and reputation as writers and teachers, would be an honor to any country, however high its standard of excellence. To unite these men into one harmonious whole, for the benefit of all, is one of the main objects which the founders of the American Surgical Association had in view when they met at Atlanta, in May, 1879. If it be said that we are striking a blow at the American Medical Association, we deny the soft impeachment. On the contrary, we shall strengthen that body by rousing it from its Rip Van Winkle slumbers, and infusing new life into it. We can hurt no society now in existence, or likely to come into existence. We can hurt only ourselves, if we fail to do our duty. We hope to make the American Surgical Association an altar upon which we may annually lay our contributions to Surgical Science, and so show to the world that we are earnest and zealous laborers in the interest of human progress and human suffering. We live in a fast age. Our heritage has come to us in pleasant places and under a propitious sky. Progress stares us everywhere in the face. The surgical profession was never so busy as it is at the present moment; never so fruitful in great and beneficent results, or in bold and daring exploits. Many of the best minds in this and other countries are at work in perfecting our knowledge, and placing it upon an immutable basis. Theory has given way to fact, and nothing that cannot withstand this test is worthy of acceptance. The whole field of surgery, from the structure of a boil upon a man's face to an amputation of the hip-joint, is undergoing revision. Operative surgery challenges the respect and admiration of the world; and, if it has not attained its finality, it is as nearly perfect as we can hope to make it. Therapeutic surgery, too, is making rapid strides; and surgical pathology was never more zealously, or more successfully, cultivated. New avenues are constantly being opened, and the importance of the study of physiology, in its relations to practical surgery, is daily becoming more and more apparent. In short, in whatever direction

we cast our eyes, nothing but the most substantial encouragement greets our vision, and urges us on to increased exertion.

Since our last meeting death has been busy in our ranks. Some of our best and most eminent members have dropped by the wayside, in the midst of their work and in the height of their usefulness. Their seats in this Association will no more be occupied by them. The list comprises the distinguished names of J. C. Hughes, of Keokuk, Iowa; W. Warren Greene, of Portland, Maine; Hugh W. Brock, of Morgantown, West Virginia; John T. Hodgen, of St. Louis; and James R. Wood, of New York. To these may be added that of H. Lenox Hodge, of Philadelphia, who died early last summer, deeply regretted wherever he was known. The death of these men, all widely known and recognized as able teachers and practitioners, is a signal loss to the profession and to the country. I would respectfully suggest that proper action be taken by the Association, expressive of our deep sorrow and of our warmest sympathy with the families of the deceased. The departure from among us of so many of our most prominent members within a few months of each other, at this early period of our organization, is a calamity of no ordinary nature.

REMARKS BY THE PRESIDENT, DR. S. D. GROSS, ON HIS RENOMINATION FOR PRESIDENT.

[At the meeting held in Philadelphia, May 31, 1882.]

Before the report shall be acted upon by the Association, I desire to offer a few remarks respecting my nomination for the office of President. While I am fully sensible of the high honor which the committee have intended to confer upon me, by placing me at the head of their ticket, I hope and trust that they may be induced to reconsider the matter, and select some other Fellow for the position. I claim no pre-emption right to this office, and I have certainly occupied it long enough. The action of the committee is at variance with the genius of our republican insti-

tutions, and the habits of the American people. Rotation in office is, always has been, and always will be, the order of the day, not only among politicians, but among scientists and professional men. It is right it should be so, for the expectation of office is an incentive to exertion. The object of my highest ambition, as it respects this Association, has been accomplished. Its success is fully assured, and I retire from the chair, which I have occupied during the last two years, with feelings of no ordinary emotion. We have done a noble work, a work which far exceeds my most sanguine expectations, and which, if I do not greatly err, is destined to pass into history.

The establishment of an Association which should bear a national name, and embody in one harmonious whole the surgical talent, experience, and wisdom of this great country, had long been one of the cherished objects of my ambition. No opportunity, however, of a favorable nature for carrying my design into effect occurred until the meeting of the American Medical Association at Atlanta, in May, 1879. Calling around me three distinguished friends, Professors Gunn, Briggs, and Dawson, all widely known as great surgeons, I disclosed to them my scheme, and succeeded at once in enlisting their hearty sympathy and support. It was agreed that on the following day, immediately after the adjournment of the Surgical Section of the Association, I should, in a brief speech, lay our plans before that body. This having been done, the meeting was organized by the appointment of the eminent and venerable Dr. Dugas, of Augusta, Professor of Surgery in the Medical College of Georgia, as Chairman, and Professor Dawson, of Cincinnati, as Secretary. Brief addresses were made by different gentlemen, all cordially approving of the objects of the meeting, but for reasons which need not now be mentioned, nothing further was done on that occasion. Before, however, parting with my immediate associates, it was determined that we should send a circular to the principal surgeons of the United States, setting forth our plans, and inviting their co-operation at a conference to be held at New York during the sitting of the American Medical Association in June, 1880. At this meeting Professor Sayre, on my

motion, was placed in the chair, and nearly fifty gentlemen registered their names, a temporary organization being effected by the adoption of a constitution and the election of officers. I had the honor to be put at the head of the list. Professor Dugas was elected first Vice-President, and the late lamented Professor James R. Wood second Vice-President. The next meeting, one also wholly of an executive nature, took place at Richmond, Virginia, in May, 1881, at which it was resolved that the first regular meeting for the transaction of scientific and practical work should be held at Coney Island the following September. Although this meeting was slimly attended, it performed useful work, and laid the foundation of the present meeting, which has eventuated in such a triumphant success.

Thus, Mr. Chairman, it will be perceived that what promises to become, if it not already is, a great and useful, and, we hope, an enduring Society, had, like all similar enterprises, a small and humble beginning, with not a few early struggles and trials. Before I resume my seat, let me again entreat the Association to withdraw my name from the list of officers.

When Dr. Gross sat down Dr. D. W. YANDELL, addressing the Chair, spoke as follows: "What is writ is writ, what is done is done. When the Nominating Committee, of which I was a member, and for which I rise to speak, met, it instinctively on the instant, and unanimously, chose the venerable surgeon, who has just spoken, for the highest office, the first place in the gift of the Association. The committee, sir, selected him as his own successor, and in doing so we felt that our action did not express the wishes of this body alone, but that it expressed the sentiment of every true man in the profession in America. Had the Constitution of the Association conferred upon us the power, we would have put on him the royal purple, and, hailing him chief among all, have bid him wear it for life; for, having worn the sceptre, we wished that the hand which had plucked such enduring renown for home surgery should continue to wield it; a hand, the skill of which an age bordering on fourscore years has not yet impaired, and the frank and cordial pressure of which no man has ever felt without feeling the better for it.

"As the founder of this Association, he opened up possibilities for surgery for which we wished to thank him.

"As the father of modern American surgery, we wished, as dutiful children, to convey to him in our report some idea of the respect and reverence we felt for him personally, no less than our appreciation of his labors in the science which he has done so much to create, to enlarge, to illustrate and adorn, and of which to-day he is so complete an embodiment. And in what we did, Mr. Chairman, we felt not that we were uttering the sentiment of the profession of this country alone, but that in tendering him fresh honors, the action would be approved by workers in surgery throughout all the world.

"Sir, the deeds of this our Fellow, whose name is now again offered you for the presidency, are great deeds, and 'great deeds cannot die. They, like the sun and moon, renew their light forever.' And we have all been blessed by looking on those of him whose term of office, having first expired, we now propose to renew.

"We are fully aware that, did Dr. Gross consult his own wishes, he would, on laying down the cares of office, not willingly resume them. But he must, in this case, yield his wishes to ours; for the united desire of the profession to which he has given his undivided life possesses, let us believe, the force of a command. Ordinarily, fathers direct their children. Here the children, through love, command the father. In this spirit, then, the Committee cannot allow Dr. Gross to decline the office which they have laid at his feet. And in a career of unsurpassed usefulness and of unequalled splendor, crowned both at home and abroad by every possible honor, the Association would fain believe that this, though the most recent, tribute to his worth shall not be esteemed by him to be the least.

"In conclusion, the Committee, acting for the Association, and speaking for the profession at large in the United States, not only declines to withdraw the name of our beloved founder, but begs here to declare that he is unanimously again chosen President of the American Surgical Association."

A MINUTE OF REGRET.

THE following minute of regret was ordered to be entered on the records of the Association at the meeting held in Philadelphia, May 31, 1882, in memory of Fellows of the Association whose death occurred since the last meeting.

The Association has learned with profound regret of the death of W. W. GREENE, J. C. HUGHES, H. W. BROCK, J. T. HODGEN, J. R. WOOD, and H. LENOX HODGE, and desires to place on record the following minute:—

In contemplating our great loss we painfully realize that it includes some of our most valued members. Scholarly attainments, brilliant achievements, ready resources, patient investigation, devotion to surgery, and genial companionship are some of the elements in the character and works of our lost brothers. While we emulate their virtues, let us inscribe on our records our appreciation of their eminent qualities and the deep sorrow which oppresses us as we realize our loss.

MOSES GUNN,
E. M. MOORE,
J. EWING MEARS,
Committee.

ADDRESS OF WELCOME

BY THE PRESIDENT,
S. D. GROSS, M.D., LL.D., D.C.L. Oxon., LL.D. Cantab.

[Delivered at the opening of the meeting held in Cincinnati, May 31, 1883.]

Allow me, as your presiding officer, to congratulate you upon your safe arrival, and to extend to you a cordial welcome to the "Queen City," the centre of medical education in the "Buckeye State," the residence of eminent medical teachers and practitioners, and the repository of the ashes of great medical men, who, during their laborious lives, added lustre to the profession, and left behind them undying names. Although nearly forty-three years have elapsed since I left Cincinnati, after a residence of seven years, I still feel as if I had some right to its citizenship and some right also to invite you to its hospitalities.

We have come hither, not as pleasure-seekers, but as members of a great body of men bent upon earnest work in the interests of our great and noble calling. It is a source of rejoicing to me, as I am sure it is to you, that, since our last meeting, our ranks have not been invaded by the "Fell Destroyer." On that occasion it was my melancholy duty to announce to you the demise of six of our Fellows, all men of distinction and of great usefulness. The meeting upon which we are about to enter cannot fail, if we may judge from our programme, to be one of unusual interest, and that our deliberations will be conducted with that celerity and the harmony of feeling which heretofore characterized them I am safe in predicting. Allow me, in concluding this brief salutatory, to direct your attention to a proposed alteration of the Constitution to increase the number of Fellows from 100 to 150. The change is an

important one, and merits your serious consideration. I also respectfully suggest that the acting Council be increased from four to seven, so as to impart greater efficiency to its work. Finally, it appears to me wise to take action upon all proposed amendments of the Constitution and By-laws on the last day of every session, instead of letting them lie over for one year, as heretofore.

VALEDICTORY ADDRESS

BY THE PRESIDENT ON HIS RETIREMENT FROM OFFICE.

[Delivered at the close of the meeting held in Cincinnati, May 31, 1883.]

LET me congratulate you, gentlemen, on the success of this, the third, annual meeting of the American Surgical Association. I have been your presiding officer from the beginning, from the inception of the Association, down to the present moment. I feel honored, and highly appreciate what you have done for me. In all your deliberations I have endeavored to act with an eye single to the interests of the Association, and with that impartiality which has always characterized my actions in all my relations to the Association.

Now, that I am about to retire from the discharge of these duties, I feel sure that the interests of the Association will be perfectly safe in your hands and in the hands of my distinguished successor. I have no greater ambition than to live in your affection and esteem, and to witness the ever increasing prosperity of the Association. Its success is closely associated with the remainder of my life. I believe that we have here an association which is destined to become a power for good in the land; and when I am dead and gone you will not, I am sure, relax in your efforts to make it, what its founders intended to make it, a great and lasting institution. God bless you, gentlemen, in all the relations of life! and may you ever be faithful to the interests of the profession and the Code of Ethics, in connection with which we have carried on our work!

THE INFLUENCE OF OPERATIONS UPON THE PROLONGATION OF LIFE AND PERMANENT RECOVERY IN CARCINOMA OF THE BREAST.

By SAMUEL W. GROSS, A.M., M.D.,
SURGEON TO, AND LECTURER ON CLINICAL SURGERY IN, THE JEFFERSON MEDICAL COLLEGE HOSPITAL, AND THE PHILADELPHIA HOSPITAL.

[Read September 13, 1881]

THE convictions are steadily gaining ground that carcinoma of the breast is curable, and that it is primarily a local affection, and not the expression of a constitutional taint, dyscrasia, or diathesis. In favor of these views—which are held by Virchow, of Berlin, Billroth, of Vienna, Fischer, of Breslau, Esmarch, of Kiel, Nussbaum, of Munich, Volkmann, of Halle, Kocher, of Berne, Erichsen, Hutchinson, Sir William Gull, Simon, Moxon, Bryant, Arnott, Payne, Green, and formerly by De Morgan and Moore, of London, and by Gross, Parker, Peters, Neftel, Richardson, Moore, and McGraw, of the United States—the course of the disease shows that visceral tumors are preceded by signs of extension of the cellular elements of the growth into the adjacent tissues and by infection of the associated lymphatic glands; and they are, moreover, sustained by the results of surgical interference, the latter of which furnish the motive for this paper.

In view of the inevitably fatal progress of carcinoma of the breast when uninfluenced by operation, the great questions to be considered from the standpoint of pathological histology are, whether the knife prevents, first, local dissemination; secondly, lymphatic involvement; and, thirdly, the development of metastatic tumors.

1. The answer to the question—Does surgical intervention prevent the invasion of the skin, connective tissue, muscles, and walls of the chest?—should be affirmative in a certain proportion of instances. Observation during life and during operations indicates that the adjacent tissues are infected in 90.82 per cent. of all instances; while of 395 cases in which this point could be traced, there was local recurrence, after removal, in 315, or 79.74 per cent. Hence extirpation precluded continuous invasion of the surrounding tissues in 10.08 per cent. of all cases, a proportion which approximates that of the permanent recoveries, as we shall see presently.

2. In regard to the second question—Does extirpation prevent infection of the associated lymphatic glands?—the reply must be that it certainly does. Thus, of 657 cases, glandular implication was witnessed in 422, or 64.23 per cent., when the patient first came under observation. In all the axillary glands were enlarged, and along with these the supraclavicular glands were involved in 27, the infraclavicular in 5, and the cervical in 3. Of 114 operations in which this point is noted, the recurrent disease was seated in the glands in 46, or 39.31 per cent., or in 24.92 per cent. less than when the affection was not influenced by interference; and the fact is noteworthy that the glandular recurrence was more frequent by 19.64 per cent. when the breast alone was removed than when it was extirpated with the glands.

3. In replying to the third question, it should be remembered that metastatic tumors are not always developed. Thus, of 740 patients dead of carcinoma, in whom the disease pursued a natural course, or who had submitted to operation, contamination of the viscera was discovered in 303, or in 40 per cent., so that more than one-half succumbed from the intensity of the local disease and the pernicious effects exerted upon the general nutrition through the loss of blood, offensive and exhausting discharges, and suffering. Of the entire number, in 252 the case was not influenced by operation, and metastatic growths were found in 157, or 62.30 per cent., while of the 488 subjected to the knife, the disease had extended to the viscera in only 146, or 30 per cent. As the course of the disease when left to itself

shows that metastases are absent in 37.70 cases out of every hundred, and as they are wanting in 70 out of every hundred of the patients dead after operation, it follows that operations prevent implication of the inner organs in 32.30 cases out of every hundred.

Having thus seen that local extension, reproduction in the glands, and the formation of metastatic tumors may be obviated by the removal of the breast, we are justified in inferring, first, that life may be prolonged, and, secondly, that a permanent recovery may be effected by surgical intervention.

1. In regard to the first deduction—namely, the extension of life—an analysis of 67 cases which pursued a natural course, indicates that the average life was 27 months. Of 224 patients, on the other hand, who died after interference with local or general reproduction, the average duration of life was 39 months; so that extirpation adds one year of life.

2. The question of permanent recovery after operation must be based upon the study of the typical course of the affection, which demonstrates that certain events which happen at certain times should determine the date at which a cure may be regarded as being assured. Volkmann[1] states that "if a year has elapsed after operation, and the most careful examination fails to detect evidences of local recurrence, enlargement of the glands, or visceral implication, we may begin to hope that a permanent result will be attained; and that a cure is generally assured if two years have elapsed, while it is rendered almost certain after the expiration of three years." As I have pointed out elsewhere,[2] the average date of death from metastases is 31.2 months; and that local reproduction is witnessed in only one case out of every two hundred, after the expiration of three years. Hence, I assume, with Volkmann, that the result may be said to be final if the patient survives over three years without local or general recurrence after the last operation, or has died of some intercurrent malady under the same conditions. Of 524 cases of

[1] Beiträge zur Chirurgie, 1875, p. 325.

[2] A Practical Treatise on Tumors of the Mammary Gland, p. 164.

scirrhous, medullary, colloid, and atrophying carcinoma, in which the result is known, 57, or 10.87 per cent., or 1 in every 9.19, fulfilled these requirements, 53 being still alive at the date of the last reports. The average duration of life after operation was six years and five months, and the disease had existed, on an average, for seventeen months and a half before surgical intervention, so that the average duration of life was seven years and ten months. Of the cases which ran a natural course, only 1.5 per cent. survived six years; while of the cures, 31.57 per cent., or nearly one-third, were free from disease after the expiration of six years. In speaking of the duration of life after operation, Sir James Paget[1] says: "I am not aware of a single clear instance of recovery: of such recovery, that is, as that the patient should live for more than ten years free from the disease, or with the disease stationary." Applying this severe test, I find that 10, or 1 in 5.7, fulfilled this requirement, 4 being free from recurrence for between ten years and one month and ten years and ten months, 2 for between eleven years and two months and eleven years and nine months, 1 for twelve years, 1 for thirteen years and eight months, 1 for fourteen years and seven months, and 1 for fifteen years and seven months.

In connection with the 57 cures, there are several interesting points which are of the first importance in influencing the prognosis after operation. In 9 instances there was repullulation. In 6 there was one recurrence; in 2 there were two reproductions, respectively, in twelve months and four years; and in 1 there were three recurrences in four years. In these three cases the subjects were free from disease for three and a half, four and a half, and twelve years after the last operation; so that, as in sarcoma of the breast, recurrent tumors should be freely extirpated as soon as they appear.

Another practical point is that glandular implication is not a bar to operation. In 18, or nearly one-third, of the 57 permanent recoveries, infected axillary glands were removed, and in several of these cases there were nodules in the skin, and the upper

[1] Lectures on Surgical Pathology, 3d ed., p. 649.

layer of the great pectoral muscle was excised. Even if the glands be permitted to remain, it does not follow that the progress of the disease may not be arrested, as their enlargement may be due to irritative hyperplasia and not to carcinomatous degeneration. In 3 cases in which they were not interfered with the patients were free from recurrence, respectively, for five years and nine months, six years and one month, and ten years and ten months. Other interesting points in regard to the prognosis after operation are that recurrence is more frequent in the axilla, by 11 per cent., when the breast alone is amputated, than when the glands are simultaneously removed, and that reproduction takes place four months earlier after the former than after the latter procedure.

In this connection I may call attention to the fact that absence of glandular implication does not afford an absolute guarantee that secondary deposits are not already present in the viscera, since in one example out of every eight metastatic tumors are formed without the intervention of infected glands.

In the final cures the operation was practised when the affection had already existed, on an average, for seventeen months and a half, or when the skin, connective tissue, and the lymphatic glands are usually implicated. Hence, it was not performed early, or at a period which is best fitted for insuring immunity from reproduction; nor was it by any means always complete. Thus, of 53 cases in which the extent of the operation is noted, the breast was amputated in 25, the breast was amputated and the axilla was cleaned out in 16, the tumor alone was extirpated in 10, and extirpation, with the removal of infected glands, was practised in 2. The disease had been observed for twenty-six months, and the recovery averaged six years one month and fourteen days, for the amputations of the mamma; it had existed for seventeen months, and the cure had lasted for five years and eight months, for the amputations of the mamma and removal of the glands; and its duration before operation was nine months and thirteen days, and the average life afterwards was five years eight months and seven days for the partial operations. Of the 53 cures, the 16 in which both the breast and the glands

were removed are alone to be regarded as having been subjected to radical operations, since every possible source of local and general recurrence should be got rid of to constitute a thorough operation; and I now make it a rule to amputate the mamma with its investing fat and skin, search for any outlaying lobules, dissect the fascia off the pectoral muscle, and open the axilla with a view to its thorough exploration, and the removal of any glands that may have escaped observation previous to interference. If the muscles are infiltrated they should be cut away with a bold hand, and the wound, especially in the vicinity of vessels, along the lymph sheaths of which extension of the disease not infrequently takes place, should be seared with the hot iron.

If such unexpected results, namely, one cure out of every nine operations and one-fifth, are attainable by procedures of which seven-tenths must be regarded as inadequate to remove all affected tissues, what degree of success may we not reasonably expect from early and radical measures? As soon as women have learned that a permanent recovery may be anticipated when the disease is in its incipiency, they will apply sooner to the surgeon for relief, and there is every reason to believe that the ratio of cures will be greatly increased.

The results that I have indicated—and they are derived from the recent and trustworthy statistics published by Winiwarter,[1] Oldekop,[2] Henry,[3] and Estlander,[4] have not been attained without a very considerable mortality, since of the 602 operations, 104, or 17.27 per cent., were fatal from the immediate effects of interference. The rate of death is about 10 per cent. for partial operations; 7.69 per cent. for amputation of the breast, and 23.10 per cent. for amputation of the mamma and extirpation of the glands. Hence partial extirpations should be discarded, as they are more fatal, possibly from aiming at primary union, than removal of the entire breast, and as they hold forth little pros-

[1] Beiträge zur Statistik der Carcinome, Stuttgart, 1878.
[2] Langenbeck's Archiv, Bd. xxiv pp. 536 and 693.
[3] Statistiche Mittheilungen über den Brustkrebs, Breslau, 1879.
[4] Revue Mensuelle de Médecine et de Chirurgie, 1880, p. 585.

pect for permanent recovery. The reason for the excessive mortality when the mamma and axillary glands are removed is to be found in the defective manner in which the axillary wound was managed in a large proportion of the cases. Thus of 187 operations of this description—and they constitute 68 per cent. of the entire number—Billroth[1] lost not less than 40, or 65 per cent. of the entire number of deaths, of which 25 were due to pyæmia and septicæmia, which were frequently combined with hemorrhage from the axillary vessels, 3 were due to hemorrhage alone, 11 to erysipelas, and 1 to pneumothorax from opening the pleural cavity. Instead of trusting to ligation of the divided veins, against which his early teachings had prejudiced him, he resorted to plugging the wound; but he states that since he has found that veins may be tied with impunity his results are improving. In the future I believe that the mortality from radical procedures will not reach 10 per cent., since of 17 cases—15 of which occurred in hospital practice—in which I amputated the entire breast and its coverings, removed the pectoral fascia, and opened the axilla, I have lost only one, the subject having been a fat woman, who was impressed with the idea that she would not recover. In 14 of these cases infected glands were extirpated, and in 3 large portions of the pectoral muscles were cut away.

From all the data contained in the paper, I believe that I am warranted in framing the following practical conclusions:—

1. That surgical intervention in carcinoma of the breast tends to retard the progress of the disease by preventing local dissemination, implication of the associated lymphatic glands, and the development of visceral tumors.

2. That local reproductions do not militate against a permanent recovery, provided they are freely excised as soon as they appear; and that lymphatic involvement does not forbid operation, since infected glands were removed in more than one-third of the examples of final cure.

3. That the subjects are almost without exception safe from local and general reproduction if three years have elapsed since the last operation.

[1] Chirurgische Klinik, Wien, 1871–76, p. 268.

4. That the risk from operations is outweighed by the benefits which accrue from them, since they not only add twelve months to life, but also cure more than one-half as many patients as they destroy.

5. That all carcinomata of the breast, if there are no evidences of metastatic tumors, and if thorough removal is practicable, should be dealt with as early as possible by amputating the entire mamma and its integuments, dissecting off the subjacent fascia, and opening the axilla with a view to its exploration and the removal of any glands which were not palpable prior to interference.

REPORT OF A CASE OF SUPPOSED SPONTANEOUS ANEURISM OF THE POSTERIOR TIBIAL ARTERY.

LIGATURE OF FEMORAL ARTERY. INCISION INTO SAC ONE MONTH AFTER, FOLLOWED BY SERIOUS HEMORRHAGE. AMPUTATION THROUGH THE THIGH. RECOVERY. ALSO, A RÉSUMÉ OF THE LITERATURE OF THE SUBJECT.

By R. A. KINLOCH, M.D.,
PROFESSOR OF SURGERY IN THE MEDICAL COLLEGE OF THE STATE OF SOUTH CAROLINA, CHARLESTON, S. C.

[Read September 13, 1881.]

CASE.—J. W. R., white, adult, aged 45, coming to me from the interior of the State, was placed in the surgical ward of the City Hospital on the 20th of June, 1880. On examination he presented a disease of the left leg, the limb being so large posteriorly from knee to ankle as to give the appearance of an enormous tumor springing from the posterior surface of the bones. There was besides much general œdema of the member. The limb was semi-flexed, quite warm to the touch, and very painful. The following were the dimensions of the leg below the knee: Greatest circumference $22\frac{3}{4}$ inches; circumference close to popliteal space 19 inches; circumference near ankle $14\frac{1}{4}$ inches; normal leg, greatest circumference 15 inches.

There was no history of injury to the part. The only recollection of any accident suggesting traumatism was of having been thrown from a wagon six or more years ago. There was no injury to the limb at that time. Three years ago trouble with the leg began, without known provocation, in the form of a "small, hard lump," in the upper portion of the calf. This increased very slowly, and had at one time, he thought, some pulsation.

The great and rapid development had been during the last seven

months. The helplessness and pain of the limb had induced him finally to seek advice. At the suggestion of Dr. Johnson, of Walhalla, he had come to Charleston to be under my care. Dr. Johnson had not watched the case for any time, but six months previously he had attended the man for an erysipelatous condition of the leg threatening suppuration, and subsequently he regarded the disease as aneurismal, and treated it with bandage and tourniquet without result. There remained some doubt in his mind as to the correctness of the diagnosis.

The tumor was semi-solid, elastic, and indistinctly fluctuating to the touch, smooth, not lobulated. The tegumentary surface was congested and of a dusky bluish color. There were large subcutaneous veins distinctly visible stretching across the swelling. There was apparently no diminution of the swelling upon direct pressure, nor was the tumor influenced by interrupting the flow of blood through the femoral artery. There was no pulsation, but when the palms of the hands were made to bear firmly upon a large extent of surface at about the region of the greatest circumference of the limb, a slight upheaving, or rather an eccentric movement of the mass, was recognized. This was not perceived when the flow through the femoral was cut off by pressure at the groin. There was no *bruit*, that I could discover, although one of my young friends thought that he recognized such. There was a trifling enlargement of some of the lymphatic glands in the groin of the affected side. The appearance of the patient was healthy; body spare; countenance indicating care and suffering; there was nothing expressive of so-called malignant cachexia. He slept badly at night because of pain in the limb; his appetite was defective; his pulse quickened.

Diagnosis.—Aneurism most probably of posterior tibial artery, or possibly of lower portion of popliteal, recently become diffusive or false; sac filled with fibrin and coagula. Many of my medical friends examined the case, and the majority of them entertained serious doubts as to the correctness of this diagnosis. Opinions varied as to the existence of an encephaloid tumor, a sarcomatous growth, or a pulsating tumor of bone. The time the tumor had existed, its mode of early development, its recent sudden and rapid progress, its uniform smooth surface, the eccentric swelling upon removal of pressure, the character of the pain complained of, and the general condition of the patient induced confidence in my own

opinion, but I could not forget how often surgeons more experienced and more capable than myself had, under similar circumstances, been led into error. The rarity of *spontaneous* aneurism in the arteries of the leg was a strong argument, with some, against the conclusion I had reached.

In view of the suffering of the patient, and the size and condition of the supposed sac, believed to be filled with coagulum, I thought the ligature of the femoral was the most promising operation. After the ligation, I proposed to aspirate the tumor with the object of confirming the diagnosis.

Operation.—January 23d. 12 M. I ligated the femoral below the apex of Scarpa's triangle. Intending to use the antiseptic catgut ligature, I was, at the required moment, much disappointed in failing to procure a proper article, I therefore employed carbolized silk, and, as commonly done, retained one end of the thread, with which to remove the loop after the ligature had cut through the vessel. For the first time in my own experience and observation in the ligation of the femoral in the continuity, I experienced an unexpected trouble from hemorrhage. Just as I passed the needle under the vessel, there came a sudden and free gush of blood. I perceived that the bleeding was arterial, and yet could scarcely believe I had injured the main vessel either with the knife or needle. The flow was soon controlled by the finger, and by an assistant pressing the femoral at the groin. I then discovered that the hemorrhage came from a branch of the main vessel, which had been ruptured in passing the needle. This vessel I secured, then tied the main ligature to the femoral immediately below the small branch. It may be interesting to remark that this accident is alluded to by the late Mr. Syme, of Edinburgh, so well known by his brilliant operations in connection with aneurismal disease. He is the only author, so far as I am aware, who mentions the fact. He says, when describing the operation on the femoral in the first edition of his Principles of Surgery: "The needle, though introduced with care and dexterity, sometimes occasions a pretty copious flow of blood, which fills the wound almost as rapidly as it is wiped out, but ceases upon the ligature being tied, and probably depending upon the injury of a small branch happening to come off at the part." The wound was closed by four silver wire sutures and dressed with carbolized oil, and a compress of cotton. I now aspirated the tumor, but failed to extract any more than a little

bloody serum. The aspirating tube was delicate, and allowed of no free escape of the contents of the sac. The curiosity on the matter of diagnosis was thus not satisfied. The patient was put to bed and given an anodyne. The limb was wrapped in several layers of cotton wadding.

Omitting the details of my clinical notes for several days, I will only state, that the operation at once afforded some relief to the tension of the tumor, but the patient was restless and slept badly after the second day.

25*th.* There was quite a rise in temperature (104° to 106°), and I felt that the wound could not be doing well.

26*th.* I removed the dressing, and discovered a circumscribed ovoid, mulberry-colored spot, two inches long by one wide, just below and a little to the outside of the wound made for ligating the femoral; also evidences of cellulitis extending from this point over an area of several inches to the outer and lower aspect of the thigh. There was evidently beginning gangrenous action. To meet this I made two free incisions through the diseased structures, and gave exit to a spoonful or two of putrescent fluid and some offensive gas. The lowest suture holding the wound, was cut and removed, and a director forced from this point through the incisions just made to the outer and lower side of the thigh. Some more offensive pus was thus liberated. After a thorough cleansing and disinfecting of the part with carbolized water, the carbolized oil dressing was re-applied. The patient was ordered quiniæ sulphatis, ℈j, opii, grs. ij, twice a day. Diet: milk and bread; brandy ℥j, three times a day. Wound to be dressed twice a day. Greatest circumference of limb 21½ inches.

February 1*st.* The patient's condition has improved. The temperature is now normal, appetite has returned, the gangrenous tissues have separated, and the cellulitis seems arrested. The greatest circumference of limb was now 20½ inches. The general treatment had been by quinine and opium with an occasional cathartic, nutritious diet, and a moderate amount of brandy.

12*th.* The patient has continued to improve. The tumor presented about the same appearance, but the subsidence of the general œdema of the limb was more marked. I was surprised this day by a condition first discovered by one of my medical friends, who had looked in at the case, which seemed to indicate a *destruction of a portion of the inner edge of the tibia,* about three inches below the

tuberosity. Through the tegumentary covering we recognized, what we believed to be, loss of substance, and there was seemingly a crescentic sharply defined excavation of the bone, one and a half inches in length. Upon slight pressure particles of bony tissue appeared to yield under the finger. I was startled by this phenomenon, and the faith in my diagnosis was a little shaken. Many medical friends examined the limb, and all agreed that the bony tissue had undergone destructive absorption in connection with the growth of the tumor. Some saw in this additional support to the idea of a sarcoma, or a cancerous disease of the bone; others now regarded the case as one of osteo-aneurism. A few thought that the pressure of an aneurism proper would explain the destruction of the osseous tissue, just as has been noticed in connection with internal aneurism encroaching on the spinal column. I could scarcely believe that the pressure of an aneurism originating behind the tibia, could be sufficient to destroy the bone while the soft tissues behind the leg had yielded so freely to the growth of the tumor; nor could I understand why, if such were the case, the inner edge merely of the bone should be destroyed, and in so restricted and precise a manner.

13th. The ligature came away this A. M. without hemorrhage. Patient's condition good. Measurement showed, however, no further diminution in size of tumor other than what might be attributed to the subsidence of the general œdema.

24th. (One month after operation.) The patient's condition was unchanged. He was quite anxious to return home into the country. A consultation was held with several of my colleagues, and it was thought advisable, before consenting to the patient's departure, to explore the tumor by an incision and clear up the question of diagnosis. In the event of the case being a sarcoma, or other tumor strictly of the bone, we proposed to amputate through the thigh. *If* the diagnosis of aneurism was sustained, we hoped to find the artery secure, when we could risk emptying the sac and treat this antiseptically. If the artery was permeable, we would secure it by means of a double ligature, or amputate through the thigh, as a *dernier* resort. The patient was chloroformed. I punctured the tumor close to the inner side of the tibia, and passed my index finger into the opening. There was no hemorrhage. I then enlarged the wound sufficiently to turn out the greater portion of the contents of the tumor. The aneurismal character of the disease

was thus determined. I proceeded to break up and turn out the remaining layers of fibrin and blood clot. Upon feeling for the crescentic-shaped ulceration of the tibia above described, I was amazed to find *the bone intact and smooth.* The apparent change in its structure was explained by the location, and the yielding upon pressure of the dense fibrinous layers and coagulum. The soft tissues forming the sac were thinner and softer at the point where we had imagined the bone to be absorbed. The tumor had evidently tended to point at this region, and the fibrinous deposit adhering closely to the inner border of the tibia, above and below the spot, had led us into error in regard to the condition of the bone. I had almost emptied the aneurismal sac, and had syringed out the immense cavity several times with carbolized water, preparatory to introducing a drainage-tube and adjusting a compress and bandage, when suddenly there came a fearful gush of arterial blood. In an instant the cavity filled. I ran my hand quickly into the sac and pressed the deeper tissue against the posterior face of the tibia, and at the same time called an assistant to press the femoral at the groin. The fearful flow was thus arrested. Carefully withdrawing my hand, I found that the pressure of the femoral commanded the bleeding. I now enlarged the opening into the sac, and endeavored to find the point where the artery communicated. The adherent layers of fibrin and clot rendered my search futile, and I was convinced that to secure the vessel at all it could only be by ligatures *en masse,* which might include other important structures, and, most likely, *not* succeed in the obliteration of the vessel, because of the suppuration and gangrenous action to be subsequently encountered. A hasty consultation was held, and amputation was determined on. This I practised close above the knee. Patient recovered rapidly.

April 5th. He was discharged from the hospital and returned home in good health.

Dissection (by Dr. Edward P. Rose) proved the lesion to be an *aneurism* of the posterior tibial artery, about two inches below its origin. The artery was pervious both above and below the opening into the sac. It had been fed by collateral branches from the femoral below the point of ligature. The opening of the sac and the detachment of the clots and fibrinous layers had occasioned the hemorrhage.

Remarks.—The rarity of spontaneous aneurism of the vessels of the leg is well known to the profession, and readily accounted for by the size and location, together with the anatomical and physiological peculiarities of these vessels. Crisp, in his tables of 501 aneurisms, reports but two cases of aneurism of the posterior tibial. Norris, in his tables showing mortality of the ligature of the femoral, records three cases, and one of these is found in Crisp's tables. Dr. D. H. Agnew, in his recent work on surgery, alludes to having seen one case in Philadelphia in 1875, in the practice of Dr. De Forest Willard. I do not propose to go further into the opinions or experience of authorities, ancient or modern, but will record at the end of this article all of the cases of this disease I have been able to find in the limited field of research to which I have had access. But here I beg to say that I use the term *spontaneous* aneurism with some hesitation, and simply to imply that in the cases reported there is a want of reasonable proof of origin from traumatism. I have admitted a few cases into the table with a supposed traumatic origin because I regarded the proof of such origin insufficient and thought it best to reserve judgment. Moreover, I recognize the fact that, upon a close investigation of the history of all the admitted spontaneous aneurisms of the larger arteries, there is frequently found to be associated the history of some antecedent blow or some undue exertion. Perhaps, in the strictest pathological sense, there is never a spontaneous aneurism of the posterior tibial artery. But practically we must take a different view of the matter.

The differential diagnosis between aneurism and other tumors, including diseased conditions of the bones, has frequently puzzled the best surgeons. In this case, perhaps, the uncertainty of diagnosis, when the patient came into my hands, was as striking as in any case upon record. The deception arising from the peculiar relation of the fibrinous layers and blood clot to the border of the tibia was singularly interesting. And, lastly, the question as to the treatment pursued may be considered as a very debatable one. I regarded ligation as the promptest means for relieving the patient of his suffering. Pressure had been tried, and now its promise was too uncertain; its attendant pain

or discomfort not likely to be borne. The ligation of the femoral I deemed worthy of trial, but I did not feel sanguine of its success. It was practised somewhat as an experimental procedure, and only because of the condition of the sac, as resulting from the length of time the disease had existed. The double ligature to the posterior tibial itself would have been the most complete surgical operation, but in the state of the limb this could hardly have been accomplished, and most certainly not without opening the sac. Had the case been seen in its earlier development, the double ligature of the artery would have been preferred to the Hunterian operation on the femoral.

The opening of the sac I was opposed to as a primary operation. As a general rule, too, surgeons are agreed upon not opening the sac after the use of the ligature. I ventured, for reasons given above, to depart from this rule a month after I had applied the ligature. The question may be asked if this practice did not cost the man his limb; or if a cure would have resulted had the sac not been interfered with. I incline to the belief that there would have been no cure; that the full establishment of the collateral circulation would have brought renewed trouble and progressive disease. The dissection proved that the artery was pervious above and below the sac, and that the tumor could thus receive an abundant supply of blood. It was but a matter of time when there would have been demanded either amputation or the old operation of Antyllus.

Table of Cases of Spontaneous Aneurism of the Posterior Tibial Artery.

No.	Date.	Sex and age.	Authority.	Reporter.	Treatment.	Result.	Remarks.
1	Feb. 1816	....	Med. Chir. Trans., vol vii. (Norris)	Travers	Ligature of femoral.	Death on 7th day.	Ligature separated on fifth day, and patient died of *hemorrhage* on the 7th day.
2	Aug. 1817	Male, 27	Dub. Journ M. & C. Sci., 1835–6, viii. p. 241.	Browne	Ligature of femoral; 2 ligatures used.	Death 41st day.	Formation of sinus along course of the Sartorius muscle.
3	Aug. 1830	Male, 59	New York Med. Jour., 1830–1, i. p. 269.	Post	Amputation of thigh.	Death 7th Sept.	Dr. Cheeseman, operator; mortification of foot Aug. 20.
4	Nov. 1830	Male, 42	Med. Gaz. Lond., 1831, viii. p. 635.	Key	Ligature of femoral.	Gangrene; death.	
5	Sept. 1832	....	South's Trans. of Chelius, vol. ii. p. 543.	Green	Ligature of femoral.	Cured.	Had a fall down stairs 12 mos. previous; pulsation of leg noticed 6 months after this, and posterior tibial did not pulsate at the ankle. This may, with doubtful propriety, be classed as *spontaneous aneurism*. Ligature separated on 39th day. Two months after operation the leg had only diminished an inch in circumference.
6	1833	....	Prov. Med. and Surg. Jour., 1833 (Crisp's Table).	Tyrell	Ligature of femoral.	Cured.	
7	Jan. 1839	Male, 48	J. Soc. Sci. Med. de Lisbon, 1839, ix., 21.	Pereira	Amputation.	Recovery	
8	Aug. 1847	Male, 35	Monthly Jour. Med. Sci., 1852, xiv. p. 368.	MacKenzie	Aneurism laid open and ligature applied above and below sac.	Cured.	Six months after operation, limb was a little curved.
9	Feb. 1851	Male, 34	Virginia Med. and Surg. Jour., 1853, i. p. 364.	Gibson	Compression by bandaging and tourniquet over the popliteal artery; limb on inclined plane.	Cured: (May 9th left hospital apparently well; but leg a little stiff).	Only cause ascribed, standing in one position a long time with muscles fixed.
10	June 1852	Male, 40	New York Med. Times, 1852, i. p. 233.	Markoe	Ligature of femoral.	Death 38 days after operation	Suppuration of sac, and peritonitis.
11	Jan. 1854	Male, 27	Tr. Path. Soc. Lond., 1863–4, vol. iii. 115.	Walton	Ligature of femoral.	Death.	May have been due to over-exertion in pushing; but he had serious disease of the *aortic valves*, which caused death.
12	Jan. 1855	Male,	Bull. Soc. de Chir. de Paris, 1859, ix. p. 258.	Lagout	Ligature of femoral.	Cured: anchylosed knee and semi-flexed leg one year after	Operator, M. Fleury. Supposed to be due to unusual exertion in pulling on boot. This not established however.

No.	Date.	Sex and age.	Authority.	Reporter.	Treatment.	Result.	Remarks.
13	Aug. 1871	Male, 32	Brit. Med. Jour., 1872. i. p. 101.	Curgeenven	Tourniquet over femoral: elastic collodion over tumor.	Cured.	Swelling came on gradually in calf of leg four months before admission.
14	May 1872	Male, 40	Sperimentale Firenze 1873, xxxi. p. 345.	Landi	Compression; injection of ergotin in tumor; ligature of femoral June 13: amputation.	Death Aug. 26.	
15	Nov. 1872	Male, 30	Lancet, Lond., 1873, ii. p. 361.	Stockwell	Tourniquet to femoral and popliteal.	Cured.	Attributed to stooping.
16	Dec. 1872	Male, 31	Lancet, Lond., 1873, ii. p. 809.	Shaue	Compression and ligature to femoral: gangrene: amputation	Recovery	Left hospital July 22, 1873, with good stump; no heart disease. Supposed stamping with foot caused disease.
17	1873	Male, 19	Dub. Jour. Med. Sci., June 1, 1877, vol. lxiii.	Richardson	Digital compression 154 hours, and then instrumental compression 11 days before cessation of pulsation.	Cured.	This patient had had a swelled leg and pain in sole of foot for two years previous. Twelve months before admission to hospital had been kicked on tuberosity of right tibia. Only ten weeks before pain began unaccountably in right leg and foot, followed by swelling at seat of disease.
18	April 1874	Male, 24	Liguria Med. Genoa, 1874, xiv. p. 404.	Arata	Compression; ligature of femoral.	Partial cure of right tibial tumor.	Anterior and posterior tibial involved—two tumors. Case not reported as finished
19	July 1875	Male, 66	Phil. Med. Times, 1874–5, v. p. 821.	Willard	Pressure on femoral in Scarpa's triangle.	.....	Case of fusiform aneurism of anterior and posterior tibial. Aneurism cured, but patient had kidney disease, which was progressing to a fatal termination.
20	1878	Male, 63	Med. Rec., N. Y., 1878, xiii. p. 318.	Crin	Compression with Esmarch's bandage.	Cured.	Five weeks after.
21	1880	Male,	Brit. Med. Jour. Lond. 1880, i. 126.	McSwiney	Laid open by mistake; attempt to tie artery; amputation	Recovery	
22	1880	Male, 45	Trans. Am. Surg. Assoc.	Kinloch	Ligature of femoral: subsequent opening of sac 30 days afterward; hemorrhage; amputation through thigh.	Recovery	In the early period of the disease had been treated by bandaging and the tourniquet without good result.

Besides the above cases we notice that Mr. Erichsen, in his Surgery, 5th English edition, vol. ii. p. 119, alludes to a preparation of a small aneurism of the posterior tibial in the Museum of St. George's Hospital. Prof. S. D. Gross, in his treatise on Surgery, vol. i. p. 793, refers to a case of aneurism of the same artery as recorded by Sir Astley Cooper.

REPORT OF SOME SURGICAL CASES.

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[Read September 13, 1881.]

THE subjoined report is offered in the hope that it may be found of some interest as a contribution to the clinical surgery of the neck.

CASE I. *Œsophagotomy without a Guide.*—S. B., a tall, thin colored woman, æt. 45, consulted me in January, 1879, on account of trouble with her throat. It began in December, 1877. She had lost nearly all her teeth, and her digestion had long been impaired. In the summer of 1878 she had a fall, and said that two of her ribs were broken; she spat up some blood. In November she began to have difficulty in deglutition; she could only swallow about half a teaspoonful of water at a time, and no tea, molasses, or other thick liquid. Solids she could not take at all. She had had no regular treatment; but a "doctor" had given her something which he called "balm of honey," which he said would burn away the disease. It gave her much pain, but no relief. For about six weeks before she came to me, her distress had been increasing greatly.

There was evidently a laryngeal growth, and on careful examination it was found impossible to pass the smallest instrument along the pharynx.

Dr. Cohen, who kindly saw and examined her with me, concurred in the opinion that some relief might be afforded by making an opening into the œsophagus, by which food could be given.

Accordingly, on March 12, the patient was etherized by Dr. Jesse Williamson, and I proceeded to the operation, having the advantage of the presence and assistance of Drs. Agnew, Brinton, S. W. Gross, Wirgman, and Hunter, and of Dr. E. T. Caswell, of Providence, R. I.; Dr. Cohen, to my regret, was detained elsewhere. I opened the œsophagus by a procedure precisely similar to that for ligation

of the common carotid artery. One vessel, probably the superior thyroid, was tied with two ligatures and divided between them. On arriving at the sheath of the vessels, the dissection was made toward the median line, mainly with the handle of the scalpel. Between the trachea in front and the vertebral column behind was the œsophagus, the heaving of which, from the respiratory efforts of the patient, gave rise to the only difficulty of the whole operation. Upon catching it with a pair of toothed forceps, and nicking it, a probe could be readily passed in and carried either way. Enlarging the opening, I then introduced a soft rubber tube about eight inches long. Into this tube a grooved ring had been slipped, and around it, fitting into the groove, a silver wire was twisted so as to form a ring on either side. The grooved ring served simply to maintain the calibre of the tube against the pressure of the wire. The tube

was kept in place by means of tapes passed through the lateral rings, and tied round the neck. By bending up the free end of the tube and slipping a ring over it, it could be readily closed. Feeding was accomplished by means of another rubber tube, having at one end a small glass funnel, and at the other a tapering glass tube; the object of all this being to introduce the nourishment with the least possible disturbance of the patient.

I am satisfied now that I made the mistake of not feeding freely enough, and that much larger quantities of nourishment could have been safely taken. The woman had been starved so long, and I was so afraid of oppressing the stomach, that I gave her only about f℥j of milk or beef-tea at a time. with f℥j of brandy; these doses of food being given at intervals of about three to five hours. To this I ascribe her steady sinking, which ended her life four days after the operation.

At the autopsy we found the posterior wall of the larynx occupied by an epitheliomatous growth, which completely barred the passage into the pharynx; although, on removal of the parts, the larynx could be drawn forwards so as to leave the channel free.

The points about this case seem to me to be the ease of the operation, and the readiness with which food could be introduced. I regret that it was not earlier resorted to, and that we were so afraid of the free administration of nourishment.

In any case, and they are not infrequent, in which a similar condition of blocking of the pharynx at its upper part existed, I should be inclined to consider the propriety, failing other means of overcoming the obstruction, of inserting a feeding-tube through the side of the canal; and of doing it before the powers of life had been so far exhausted as to lessen materially the amount of benefit that could be hoped for.

Possibly this was one of the instances in which some of the progressive surgeons of our day would have excised the larynx. But against such a plan as this, it seems to me, would have been the fact that the voice was so little interfered with, being husky indeed, but its power only slightly impaired. And of the two procedures there can be no question that the mere opening of the œsophagus is by far less dangerous for the patient, as well as less difficult for the surgeon.

I am aware that this operation (œsophagotomy without a guide) has been done before, but only very rarely. I regret that, for want of time, it is impossible for me to present a list of the cases of this kind hitherto recorded, for publication with this paper.

Case II. *Wound of the Soft Palate.*—November 19, 1880, a boy, æt. 3, was brought into the Episcopal Hospital at the time of my daily visit, having fallen on the end of a tin horn which he was blowing, and sustained a severe injury of the soft palate. On examination it was found that the soft palate had been torn away in the middle from the hard, and hung down over the root of the tongue, by a rather thick strip of tissue on either side. A very large gap was thus left in the roof of the mouth, and the raw surface of the ruptured portion of the soft palate lay, looking like a detached tonsil, on the dorsum of the tongue. The child was not apparently suffering much, and there was but slight bleeding.

Deeming it needful to sew up the wound, I went into the city to obtain suitable needles, etc., and Dr. Cohen kindly returned with me, and gave me his aid.

As the operation had to be done by artificial light, chloroform was carefully administered; and, after much difficulty, I succeeded

in adjusting three sutures of fine iron wire, one in the middle and one at either side, closing the gap except at one point. The difficulty was due partly to the small space for working, at such a depth, and partly to the ready yielding of the tissues, the wire tearing through even the seemingly firm edge of the hard palate. The sutures were allowed to remain in place for three days, and were then easily divided and removed. The little fellow was very brave, and bore all that was done with singular steadiness for so young a child. Adhesion took place very satisfactorily, and in less than a week the condition of the parts was such as to warrant the dismissal of the case, the parents promising to bring him back if any evidence of further trouble should arise.

Case III. *Infra-hyoid Pharyngotomy.*—This subject is, perhaps, more legitimately within the province of those who confine their practice to laryngeal cases; but I venture to bring it forward here because it may happen to the general surgeon as well as to the specialist to be called upon occasionally to deal with laryngeal growths, or perhaps with foreign bodies.

It happened to me, two or three years since, to have in my wards at the Episcopal Hospital a child five years of age, who had always been hoarse from the presence of a growth in the larynx, situated on the right vocal cord. Repeated attempts at dealing with it from above, through the mouth, having proved abortive, I made trial, with Dr. Cohen's assistance, of the method of opening into the hyo-thyroid membrane, by a transverse incision. The operation presented no difficulty whatever; upon the division of the muscles, the membrane could be stretched out by depressing the larynx, and access readily obtained to the pharyngeal cavity. But from the small size and narrowness of the parts, it was impossible to get such a view as to enable us to attack the growth in the larynx. The wound healed very readily, and the child, although unrelieved, was sent home in about two weeks, as well as before the operation.

In the dead adult, a most satisfactory view of the interior of the larynx, from above, can be obtained by this procedure; and it seems to me that it is applicable to grown persons only. Since the experience above detailed, I have tested the matter upon the bodies of several children, with the same result. Others may perhaps be more successful; but this has been my observation so far, and I venture to place it upon record.

ON SANITARY CONDITIONS IN RELATION TO THE TREATMENT OF SURGICAL OPERATIONS AND INJURIES.

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[Read May 31, 1882.]

It is probable that, in selecting for discussion the subject of "Sanitary Conditions in Relation to the Treatment of Surgical Operations and Injuries," assigned to me by our honored President, reference was had exclusively to external and local conditions, rather than to individual peculiarities of temperament or constitution, which exert an important influence on the results of operations. And yet, as some of these peculiarities may be the product of the hygienic conditions under which the individuals may have previously lived, and to which they may be exposed at the time of an operation, it may not be out of place briefly to notice some of them in this connection.

It was fitly observed by Mr. Erichsen, in his address on the opening of the Surgical Section of the recent International Medical Congress in London, that "in any discussion on the best methods of securing primary union in wounds, we should not fail to recognize, in the general constitutional state of the patient, a most important factor in this direction; and we should be taking a narrow view of this many-sided question, if we did not give due weight to the influence of those hygienic conditions which, if faulty, are inimical or destructive to the due performance of those actions which are necessary for the maintenance of the organism in a healthy state, and for the proper nutrition and

repair of the tissues of the body." "Is there not a fear," he asked, "that, in some of the modern systems of treating wounds, we are in danger of expending all our precautions in preventing local and ignoring the risk of constitutional infection?"

I shall accordingly deem it not inappropriate to allude briefly, in the course of my remarks, to the modes, so far as they can be determined, by which the results of operations are influenced by peculiarities of each patient's constitution, mental and physical; and by the hygienic conditions existing before, as well as those which exist during and after the operation.

Eight years ago, Mr. Erichsen expressed the opinion that the science of surgery had not advanced proportionately with the art, and that the results of all operations, especially those by which life is imperilled, are not as satisfactory as they should be—the skill in the performance having far outstripped the success in the result.[1] It is probable that this distinguished surgeon would slightly modify this general statement if he had occasion to revise and re-edit his Lectures on Hospitalism. For trustworthy statistics show a marvellous recent diminution in the death-rate of operations involving abdominal section—notably, the operation of ovariotomy—a diminution due, in some measure, to improved methods of operative procedure, which have now probably reached final perfection; but in a yet larger measure to more careful and systematic attention to sanitary conditions, including the employment of antiseptic precautions during and after the operation. He would, doubtless, still affirm that "the mortality is excessive after amputations; that it has not materially diminished of late years, and is particularly great in hospital practice, where it is probably dependent on removable causes, and hence might be materially reduced, if these could be determined and their recurrence prevented." He regards this continued high rate of mortality after amputations as very significant when contrasted with the decided decrease of the general mortality of London and many other large towns during the last third of a century. It seems to show that the "sanitary im-

[1] Hospitalism, by John Eric Erichsen, London, 1874.

provements in hospitals have not kept pace with that of the towns in which they are situated." It is probable that this reproach may justly be applied to many hospitals; but I doubt whether it can fairly attach to all city hospitals which may yet have a rather high rate for its amputation cases. There is an obvious source of fallacy in drawing such conclusions from the mortality tables.

The influences which determine the results of operations are so numerous and varied that it would be illogical to select any one of them—such as the locality in which the operations are performed—and attribute the results to that influence alone or chiefly, on the arbitrary assumption that if we compare a sufficiently large number of cases, all other influences may be made to neutralize each other. Freedom from septic diseases would be a much more reliable test of the sanitary condition of a hospital than any derived from its mortality returns. "In every case of pyæmia, erysipelas, and the like," says Sir James Paget, "we ought to work until we can discover its probable origin. We should have the strongest feeling that these diseases are not spontaneous nor inevitable." So far, therefore, as septic complications are concerned, formerly the most fruitful cause of mortality after operations, we have the best reasons for believing that they are dependent upon hygienic defects which are not inevitable, but may be removed by judicious sanitation.

Why then has not the mortality after amputations been reduced in a somewhat corresponding ratio with the marvellous diminution of the death-rate of ovariotomy? Is a death-rate of about 22 per cent. for all amputations above the wrist and ankle the best that can be expected among those classes of the population who are the usual subjects of hospital operations? Or is this high mortality peculiar to hospitals, and a necessary result of occult unhealthy influences inseparable from the aggregation of sick persons, even when septic conditions are prevented? Such are some of the questions which must engage our attention as elements of the general discussion; and although it may not be possible to furnish a ready answer to them, the inquiry will not be without interest and value. We shall, at least, arrive

at this negative conclusion, that an attempted comparison of the mortuary statistics of hospitals with those of private practice will not furnish a satisfactory solution of this problem.

The prime condition of the healthy repair of wounds is, of course, a fit state of the blood, which implies the due performance of the blood-making and blood-purifying functions, and thus has relation to whatever can affect the process of digestion, the absorption and assimilation of its products, and the elimination of waste material by respiration and the other depuratory processes. It involves the necessity of a regular and constant supply of fresh and pure air with abundance of sunlight, uncontaminated water for drinking, food of suitable quality, taken at regular intervals, in appropriate quantities, regulation of the action of the skin by attention to bodily cleanliness and by suitable clothing adapted to the exigencies of season and climate, a wise government of the intellectual and moral nature, and such an adjustment of the general system to the condition of enforced abstinence from muscular exercise as may be necessary to maintain the healthy state of the other functions under the necessary restraints of the sick-chamber. The importance of each and all of these factors of health is generally conceded in terms, but in actual practice some of them are overlooked to an extent that may perhaps have an important influence on the issue of a case of severe injury. It is not very unusual to hear the opinion expressed that the issue of a grave surgical operation, which has been well executed, depends mainly upon inappreciable peculiarities of the patient's constitution. Before taking refuge in an explanation which rests upon confessed ignorance, we are bound to see that in any given case all the hygienic conditions that might influence the physiological processes connected with nutrition have been faithfully carried out.

First, as to the supply of fresh air: While the rule of Dr. Parkes gives for persons in health the very liberal estimate of 3000 cubic feet per head per hour as necessary to keep the atmosphere of an inhabited room within the limits of permissible impurity, he states that the wards in the London Fever Hospital were not free from odor when 3720 cubic feet per head per hour

were passing in; and that in surgical wards, with patients having open wounds, the allowance must be considerably increased. When hospital gangrene, pyæmia, or erysipelas is spreading, at least 6000 cubic feet must be given; or, in other words, the supply must be almost unlimited. For the protection of others, patients affected with septic diseases should be isolated. For their own protection they should have an unlimited supply of fresh air. The large experience of Sir James Paget led him to the conclusion that there was but one thing which he could call remedial for that form of blood-poisoning which is so often the bane of operative surgery, and that was a "profuse supply of fresh air." He declared that in the three most remarkable cases of cure he had seen, "the patients might be said to have lain day and night in the wind."

Dr. W. A. Hammond states that he thinks he has noticed that wounds heal with greater rapidity when sunlight is allowed to reach them directly, than when they are kept habitually covered; and adds, that Ribes makes a similar statement.[1]

The accuracy of this conclusion may fairly be questioned, in view of the overwhelming amount of evidence we now have of the rapidity with which wounds heal under certain kinds of antiseptic dressings, without having been once opened. But as to the general sanitary value of solar light to the sick and wounded patients, there can be no doubt, and it should be kept prominently in view in plans of hospital construction.

Florence Nightingale has made some admirable observations on this subject, which are well worthy of careful consideration. They will be found in both of her instructive little works, the *Notes on Nursing* and the *Notes on Hospitals*. This intelligent and generally accurate observer remarks, in the *Notes on Nursing*, that "It is curious to observe how almost all patients lie with their faces turned to the light, exactly as plants make their way towards the light. A patient will complain that it gives him pain lying on that side. 'Then why *do* you lie on that side?'

[1] W. A. Hammond, M.D., Surgeon-General U. S. Army, Treatise on Hygiene, 1883.

He does not know, but we do. It is because it is the side towards the window."

Little need be said as to the importance of "wholesome" water except this, that water which may be "usable" for persons in health, may be positively injurious for persons in whom incipient septic complications reveal themselves by gastric and intestinal troubles; and that, therefore, the quality of the water-supply, and the condition of the pipes as to the possibility of contamination with sewer air, should be a matter of almost daily investigation in hospitals. It is well known that limpid and sparkling water, perfectly agreeable to the taste, may yet be so charged with the specific poisons of some of the zymotic diseases as to become an efficient means of their propagation, and that we have no positive test of their presence except such as is derived from clinical observation. It has not been certainly demonstrated that water polluted with a small amount of non-specific organic matter in process of decay, is as potential a cause of disease as is generally and wisely assumed. But there is some evidence in favor of this conclusion; and some of the reasons which render it probable, apply with particular force to cases in which an extraordinary demand is made upon the reparative forces of the system, and when a small addition to the amount of impurities to be excreted may seriously derange the blood-purifying process.

The necessity of a supporting diet in the case of wounded subjects is now universally recognized, but beyond this broad fact and the general principle that such diet should represent a combination of albuminoids, fat, saccharine matter, and the blood-salts in proportions approximately equivalent to those in which they exist in cow's milk—itself an excellent diet for the early stages of severe injuries during the period of febrile reaction—there is not much to be said in the present state of our knowledge. It is probable that we have yet much to learn with reference to the judicious regulation of diet for wounded patients from day to day. In the opinion of as careful an observer as the late Dr. Parkes, "there can be but little doubt that the dietetic treatment of disease is destined to be the great work of the future."

In the mean time, until our knowledge is more advanced, and we can practically use the physiological facts now being gradually discovered, he thinks it may be a good rule in disease to consider the diet of health, if it can be taken by the patient, to be the best adapted for nutrition in disease, and that we should not deviate from this rule, except on some intelligible and certain ground. One such intelligible ground, in the case of the class of patients in a public hospital who have undergone severe operations by which life is imperilled, is to allow frequent deviations from the fixed scales of diet which must, of necessity, be enforced upon other classes of hospital inmates.

The well-known influence of the emotions over the quality as well as the quantity of the secretions, renders it more than probable that mental or emotional strain may exercise a potent influence for evil over the nutritive processes. This has often been noticed among the laboring classes in times of great financial disasters leading to the reduction of the wages of labor below the limit of a healthy support for a family, although in such a case the effect is not wholly due to mental depression, but in a large degree, perhaps, to deterioration of diet both in quantity and quality.

Again; the notable paradox of a better prognosis in amputations for long-standing disease than in those performed for recent injuries, may find a partial explanation in the opportunity which protracted disease gives for quieting that mental distress which is so overpowering in the face of a great and unexpected disaster, and for restoring the harmonious balance of the animal functions which a sudden injury received by a person in vigorous health so rudely disturbs. "Such persons," says Paget, "have to give up many of the customs by which they sustained the health that was fit for pleasure or work. Especially they have to give up the active mental and bodily pursuits in which they excreted largely the refuse matter of their foods and their tissue waste." Moreover, they have to bear the shock of the operation as well as that of the injury; but we shall presently see that a particular mode of preliminary antiseptic treatment may avail to remove this last trouble, so far, at least, as that time is given for the

entire subsidence of all the physical effects of shock before the operation is undertaken, while yet inflammation and traumatic fever are averted. This precious truce, which sometimes may be advantageously extended for a week or longer, may also be utilized for counteracting in some measure the effects of previous exposure to insanitary influences connected with bad hygienic surroundings.

It is the opinion of careful and judicious surgeons that "the majority of the preventable calamities of surgery are due to the oversight of personal defects in the patients operated on; defects in the habits, the constitution, or the previous diseases which ought to have been ascertained before the risk of the operation was incurred."[1] Sir James Paget, who makes this remark, cites a number of illustrative cases, such as the removal of a small cyst on the scalp being followed by fatal erysipelas in a patient who had unsuspected albuminuria, and an amputation of a finger followed by fatal spreading cellular inflammation in a person of intemperate habits, which were not inquired into by the surgeon. Many such examples might be cited to illustrate the truth of the general proposition that the result of operation and injuries are often more determined by conditions which precede, than by those which attend or follow their occurrence. But the general fact is too well known to need further remark. I only desire to say that when circumstances admit of delay in the case of an operation, the depressing influence of antecedent unfavorable sanitary conditions may be lessened by the substitution of a better hygiene before the operation is performed. Upon a proper regulation of diet, upon the personal cleanliness of the patient and the purity of the air which he breathes, will depend the decision of the issue as to whether the operation shall be followed by a safe healing process or by a low and diffuse form of inflammation, often the precursor of secondary manifestations of a fatal blood-poisoning.

If the antecedent insanitary conditions exert an unfavorable influence on the success of operations, how much more potent

[1] Sir James Paget, loc. cit.

for evil they must be when they are continued during and after the operation. A man who, in tolerable health, might have resisted the injurious effects of close air and animal effluvia, will almost inevitably succumb to such influences acting upon a system prostrated by the shock of a severe injury. It is needless, therefore, to say that the most scrupulous attention to personal hygiene and to the hygiene of the house and its surroundings, especially in the case of large aggregations of persons in hospitals, is an indispensable condition of securing the best results in the treatment of surgical operations and injuries. Thus Spencer Wells, noting the variations in the relative mortality of ovariotomy in hospital and in private practice in the five groups of his first 500 cases, the advantage being sometimes for the hospital and sometimes for private practice, observes that it is well worthy of remark that the periods of good and indifferent results in hospital have corresponded with changes in its sanitary condition. After emptying the hospital for a month or more, and thoroughly cleansing, painting, and lime washing the walls, a period of almost uninterrupted success has followed. Then what was called "a run of bad luck" set in, clearly attributable to crowding, to some neglect in purifying bedding, or to contagion or infection.

The relative frequency of *shock* as a cause of death after amputations, is probably greater in private practice than in hospitals. Thus of 160 deaths from amputations in private practice, tabulated by Sir James Simpson, the largest number, namely, 63, was ascribed to shock, 56 having been traumatic and 7 pathological amputations. On the other hand, Erichsen reports 21 deaths from shock out of 90 total deaths from primary amputations for injury, in four metropolitan hospitals; or a percentage of 23.33, as against 39.37 per cent. in Simpson's collection from private practice.

Out of 180 deaths after amputations in the Massachusetts General Hospital between 1823 and 1871, as reported by Chadwick, only 8, or the very small proportion 4.44 per cent., are set down to shock. But it should be observed that 21 deaths, amounting to 11.66 per cent., are ascribed to "collapse," a term

usually employed synonymously with shock. But even if all these were assigned to shock, the percentage would still be only 16.11, and ought probably to be increased, for it is stated that three deaths after primary amputation at the knee for injury were all from "exhaustion," one in two hours, one in one day, and one in twenty-three days. One would suppose that shock must have been an element of the exhaustion in the first two cases. But after making due corrections to allow a comparison with Simpson's tabulated cases, it still appears that shock, as a cause of death after major operations, is relatively less frequent in hospitals than in private practice, or at least that this was the case some years ago, when the hospitals were more liable to septic influences, which, causing a large number of deaths, reduced the relative proportion of deaths from shock.

Notwithstanding the value, in other respects, of the researches of Brown-Séquard, Jaccoud, and Weir Mitchell and his associates, Morehouse and Keen, the inquiries of modern pathologists as to the mode in which shock is produced have led to no practical results in respect of the prevention of this condition. Erichsen makes the suggestive remark, that, as in amputations death is not seldom due to unavoidable causes not connected with the operation, though not, perhaps, discovered until after it, so "shock," as an assigned cause of death after amputations, may be a cloak of ignorance and require explanation, whereas pyæmia or erysipelas (especially the former), indeed every kind of septic disease may be considered as directly due to the operation.

It is in this connection that I would again invite attention to a very suggestive clinical lecture by Dr. Stephen Smith, of New York, published in a recent number of the *Medical News*,[1] on the influence of *antiseptics* on the periods of amputation after crushing injuries. It is now, he says, a matter of every day's experience, that carbolic acid constantly applied to crushed tissues, as in irrigation, will arrest all tendency, both to putrefaction and to inflammation. He is accordingly able to prolong almost indefinitely the primary or ante-pyretic period, and thus abolishes

[1] Medical News, April 15, 1882. Philadelphia: H. C. Lea's Son & Co.

the "intermediary" or inflammatory period, by the use of the agent in question. This agent, thus used, not for curative but for preventive purposes, and fulfilling that end by reason of its presumably germicidal effect, may legitimately be considered as coming within the scope of sanitary conditions. Time is thus given for the complete subsidence of the shock of injury, and for other changes which may exert a favorable influence on the result of an operation to be subsequently performed.

In view of the gratifying success which has attended amputations and excisions of the hip-joint for chronic disease, and in tertiary traumatic cases in the rare occasions in which a patient with a penetrating wound of that joint survives the primary and intermediary periods, and of the almost certain fatality attending earlier interference under ordinary circumstances, one feels anxious to put Smith's method to the severe test of a hip-joint amputation for a recent crushing or gunshot injury.

There is a general agreement as to the depressing influence of cold in increasing the severity and danger of shock, and the suggestion of Dr. Noeggerath, of New York, to apply systematically artificial heat both during and after the operation, is undoubtedly wise. Spencer Wells and Knowsley Thornton have independently come to the conclusion that the Listerian spray has sometimes done harm by its chilling influence.

Until within a few years, *septic complications* have usually been the most fruitful source of mortality after operations in hospitals, and that is often the case even now. Thus, according to the tables of Bryant (1859), pyæmia was the chief cause of death in all classes of amputations, except the secondary[1] traumatic, in which exhaustion was the most frequent determining cause. Thus, pyæmia was the cause of death in:—

Pathological amputations to the extent of 43 per cent. of fatal cases, or 5.4 per cent. of all cases.

Amputations of expediency to the extent of 60 per cent. of fatal cases, or 18 per cent. of all cases.

[1] The "Secondary" of Bryant include the "Tertiary" of recent classification.

Primary traumatic to the extent of 43 per cent. of fatal cases, or 16 per cent. of all cases.

Secondary traumatic to the extent of 25 per cent. of fatal cases, and exhaustion in 60 per cent. of all cases.

At a later date (1874) Erichsen gives the following table, representing the summary results of amputations in four metropolitan hospitals, in which the deaths from pyæmia and shock are recorded for various periods between 1861 and 1872:—

Hospital and Years.	Total.	Died.	Pyæmia.	Shock	Percentage.
Primary Amputation—					
A. 1866–70	21	13	4	6	62.
B. 1861–72	140	67	24	8	47.8
C. 1869–70	18	8	2	5	44.4
D. 1867–71	8	2	0	2	25.
Total primary . .	187	90	30	21	48.
Secondary—					
A. 1866–70	20	16	9	2	80.
B. 1861–72	53	30	10	1	56.6
C. 1869–70	5	1	1	0	20.
D. 1867–71	6	3	2	0	50.
Total secondary .	84	50	22	3	59.5
For Disease—					
A. 1866–70	89	31	15	0	34.9
B. 1861–72	215	56	16	0	26.
C. 1869–70	17	5	0	0	29.4
D. 1867–71	29	6	3	0	20.6
Total for disease .	350	98	34	0	28.00
Total of all amputations .	621	238	86	24	38.3

Mr. Erichsen's own personal experience in eighty cases of amputation (excluding all partial amputations of hand and foot) performed in University College Hospital from July 1, 1870, to December 1, 1873, gives a percentage of mortality of 26.2; the deaths from pyæmia and erysipelas being 47.6 per cent. of the fatal cases, and 12.5 per cent. of the total number of amputations. This was exclusive of the deaths from exhaustion, in regard to which there may be a doubt as to its dependence on hospital influences.

These results were reported in 1874 by this eminent surgeon as a fair specimen of the best results that had been obtained in the London hospitals at that date. Assuming that the percentage of septic complications may be taken as a measure of the sanitary condition of hospitals, the exhibit was far from satisfactory. And if this were the best that city hospitals could do, the argument of Sir James Simpson would be unanswerable. But Mr. Erichsen, though adopting the suggestive term "Hospitalism" as the title of his essay, was careful to disclaim the belief that this excessive mortality was a necessary incident of hospital surgery, and to express the opinion that it was probably dependent on removable causes, and hence might be materially reduced, if these could be determined and their occurrence prevented. It is true that in discussing the causes of "runs of good and bad luck," he ascribes their occurrence, "in many cases," to the prevalence or absence of epidemic septic influence at any given period, but the comparative infrequency of such outbreaks in tolerably well regulated hospitals at the present day casts a shade of suspicion on the accuracy of this mode of interpreting the facts in the past, and makes it far more probable that when they do occur, they are due to defects of sanitation, which, when discovered, may be readily removed. That they are now ordinarily averted to a much greater extent than formerly, will clearly appear from facts stated by Mr. Savory in his address on surgery before the British Medical Association in August, 1879, just five years after the publication of Erichsen's Lectures on Hospitalism. He cited the following statistics of blood-poisoning after operation or injury in St. Bartholomew's Hospital for the three years of 1876, 1877, and 1878:—

Cases of injury	2862	Cases of operation	1235
Deaths after injury	7.47 p.c.	Deaths after operation	5.82 p.c.
Cases of pyæmia after injury	0.29 "	Pyæmia after operation	0.89 "
Cases of erysipelas "	0.76 "	Erysipelas after operation	3.07 "
Deaths from pyæmia after injury	0.29 "	Deaths from pyæmia after operation	0.80 "
Cases of blood-poisoning after injury	1.05 "	Cases of blood-poisoning after operation	3.96 "
Death from blood-poisoning after injury	0.42 "	Deaths from blood-poisoning after operation	1.44 "

If, now, we inquire by what particular means these greatly improved results of hospital surgery were achieved, let us hear Mr. Savory's answer in his own language:—

"Each one of the surgeons of St. Bartholomew's, perhaps, carries out certain details in some way different from the others; but on the chief objects in view, we are undoubtedly in accord. We aim at the most scrupulous cleanliness in the most comprehensive sense of the term. We strive to secure this by all possible means. *We use very freely antiseptics of various kinds.*" (This statement deserves notice, inasmuch as one of the objects of the address was, to depreciate the value of the special method of using antiseptics advocated by Prof. Lister.)

"And, with cleanliness, we attach, for the most part, the highest importance to rest. We are careful to disturb wounds during the process of repair as little as possible. Cleanliness, in its surgical sense, and rest, in its physiological sense, may be said to be the leading aims. But we are by no means satisfied with directing attention only to wounds. We are most jealous of the state of the atmosphere of our wards. We keep the air as pure as possible. We have no particular or patent system of ventilation. Ventilation is effected only by open windows and large chimneys. But we are proud of the habitual state of our wards, even when most closed, as during the night time. We should not, for a moment, tolerate any unpleasant or suspicious odor. We are careful to avoid, as far as practicable, any tendency to over-crowding of wounds in the same ward, and each patient has from 1100 to 1400 cubic feet of space. Lastly, we attach the highest importance to the state of health and condition of the patient before operation; and never, where we have choice and opportunity, do we inflict an injury without previous inquiry in this direction very fully carried out."

A yet greater triumph of hospital sanitation was achieved in this city (Philadelphia), and is recorded in an interesting volume on "Surgery in the Pennsylvania Hospital" by Drs. Thomas G. Morton and William Hunt, Surgeons to the Hospital. (J. B. Lippincott & Co., 1880.)

It is stated that from the spring of 1875 to the same period in 1879, a space of four years, there were performed 108 amputations upon 100 patients. Of this number, only seventeen died,

and five of these deaths took place within the first thirty-six hours following the admission of the patient, and in each instance from recurring or continuous shock. Of the remaining twelve deaths, four were from tetanus, two from exhaustion, albuminuria, etc., three from secondary hemorrhage, two from delirium tremens, and one from serous cerebral effusion.

It is added that during the past five years, from May, 1874, to 1879, *there has not been a single case of pyæmia*, the most carefully conducted *post-mortem* examinations by Dr. Longstreth having failed to demonstrate even an approach to this malady. This result is attributed to the very perfect system of forced ventilation by the fan, which, by day and by night, from one year's end to the other, is constantly in operation, to scrupulous cleanliness of the wards, to the free use of carbolic acid in the dressings, and to an admirable system of dressing wounds with flowing water delivered from Dr. Morton's "Hospital Dressing Carriage," which has enabled the surgeons to dispense with all basins and sponges, and thus to avert the contamination so likely to occur when they are used.[1]

I am indebted to the kindness of my friend, Dr. Wm. S. Forbes, and to Dr. Houston Mifflin for the statistics of amputations practised in this hospital from June 1, 1879, to April 15, 1882, which show, out of twenty-eight amputations, four deaths from pyæmia, besides one from diffuse cellulitis, and two from prolonged shock. The fatal cases were cases of "railroad crush;" a fact which goes far to relieve the hospital, with so good a previous record, from the opprobrium of so large a percentage of deaths from septic complications. Certainly, with so small an absolute number scattered over three years, there can be no question of epidemic influence. Possibly, the hospital authorities may have rested on the laurels previously won and have relaxed a little of that untiring vigilance which is the price of hospital salubrity. For, as was well said by John Simon, who combines, with the special knowledge of an expert in sanitary science, a large experience in hospital practice, "that which makes the

[1] See also the remarks by Dr. William Hunt on Hospitalism in the same volume, p. 2.

healthiest house, makes likewise the healthiest hospital—the same fastidious and universal cleanliness, the same never-ceasing vigilance against the thousand forms in which dirt may disguise itself in air, and soil and water, in walls and floors and ceilings, in dress and bedding and furniture, in pots and pans and pails, in sinks and drains and dust-bins. It is but the same principle of management, but with immeasurably greater vigilance and skill, for the establishment which has to be kept in such exquisite perfection of cleanliness, is an establishment which never rests from fouling itself. Nor are there any products of its foulness—not even the least odorous of such products—which ought not to be regarded as poisons. Above all, this applies to the fouling of the air within hospital wards by exhalations from the persons of the sick, so that for hospital hygiene, ventilation requires pre-eminent regard."[1]

As illustrating the disastrous effects traceable to a previously altogether unsuspected source of mischief, Mr. Simon states that in one division of the Vienna Lying-in-Hospital an immense mortality from puerperal fever was believed by Semelweiss, the head of the department, to depend upon an infection of which "the real source was to be found in the hands of the medical men in attendance contaminated with cadaveric poison." The other division of the hospital (reserved for the practical instruction of midwives, whose training does not require them to be brought in contact with dead bodies) suffered only about a tenth part as much as the first, although inferior to it in size and airiness of its wards. By the use of sanitary precautions, Dr. Semelweiss reduced the mortality from $13\frac{2}{3}$ per cent. to $1\frac{1}{3}$.

I am indebted to the courtesy of Dr. S. R. Knight for the statistics of amputations performed in the Episcopal Hospital of Philadelphia from 1875 to 1882. The total number of amputations was eighty, with seventeen deaths, of which only one was from pyæmia, a majority being from shock and exhaustion. But Dr. Knight states that there were during this time some other cases of septic complications in connection with head injuries.

[1] J. Simon, 6th Report of the Medical Officer of the Privy Council, London, 1863.

The exhaustive investigation made in 1862 by Bristowe and Holmes into the sanitary condition of the hospitals of the United Kingdom and of those of Paris, under the directions of the medical officer of the Privy Council, led them to the conclusion that "whenever surgical diseases presenting open sores (especially operation cases and cases of accident) are received, hospital diseases, such as erysipelas, pyæmia, and phagedæna, are likely to arise. The liability of these affections to originate and to spread is considerably influenced by concentration of 'traumatic' atmosphere, but their development and spread are both chiefly dependent on want of cleanliness, bad drainage, over-crowding, defective ventilation, and the like." They saw abundant reason to satisfy themselves that the health of hospitals is influenced in a far greater degree by conditions belonging to hospitals themselves, than by conditions of external atmosphere, of site, of soil and the like, and that the healthiness of hospitals is less dependent on the form and size and distribution of wards than it is on ventilation, drainage, cleanliness, and proportion of inmates to space. A hospital of defective construction may, by careful attention to these latter conditions, be rendered, even in a large town, comparatively healthy; and a hospital built on the most approved plan and occupying the choicest site, may be rendered, in the highest degree, unhealthy by their neglect.[1]

It is well known that the value of the statistics of amputations in private practice collected by Sir James Simpson and published in his work on "Hospitalism" in 1872, has been sharply impugned by numerous opponents, notably Mr. Holmes and Mr. Callender. Nevertheless the impression left on the public mind is exhibited in the tendency observed of late to substitute small for large hospitals, or to construct the latter on the pavilion plan, with the restriction to a single story, or to two at most. Burdett, in his work on "Cottage Hospitals," published in 1877, after stating that the best results obtained from hospital practice, as reported by Mr. Erichsen, gave a death rate for amputations

[1] The Hospitals of the United Kingdom, by Dr. Bristowe and Mr. T. Holmes. Appendix to the Sixth Report of the Medical Officer of the Privy Council, 1863.

of 25 per cent., adds that 326 amputations in 61 cottage hospitals gave a mortality only a little over 17 per cent. He further stated that of 241 cases of amputation, with 45 deaths, in which the cause of death was reported, there were five cases of septic disease (four of pyæmia and one of septicæmia)—that is, only 11.1 per cent. of all the fatal cases. In no case of amputation for disease did pyæmia or septicæmia exist in the cottage hospitals, whereas in Mr. Erichsen's cases there was as large a percentage of deaths from pyæmia after pathological amputations as after primary amputations for injury.

It is curious, and not without significance, that Burdett, while earnestly advocating cottage hospitals for the sick and wounded, is of opinion that Dr. Mathews Duncan has conclusively refuted the objections of those who decry maternity hospitals, and say that they ought to be abandoned and small cottage hospitals substituted for them. He shows that the mortality of the Dublin Rotunda, one of the largest maternity hospitals, with a most complete set of statistics, will challenge comparison with that of any small hospital or that of any private practitioner whose statistics can be confidently relied upon.

I have, within the last few months, made an effort to collect statistics of amputations in private practice, and especially from the practice of physicians along the lines of railroads, presenting, therefore, an unusually large proportion of primary operations for railroad injuries. The number of cases is not nearly as large as I expected to obtain; but the large proportion of recent and severe injuries makes the record fairly comparable with the experience of city hospitals. The total number of amputations amounted to 686, with 100 deaths, or 14.57 per cent. They were distributed as follows:—

Thigh Cases (*including two cases of Hip-joint*).

	Cases.	Deaths.	Percentage.
Primary for injury . . .	74	19	25.67
Secondary for injury . . .	29	6	20.68
Pathological	68	9	13.23
Total	171	34	19.88

Leg—including Disarticulations at the Knee.

	Cases.	Deaths.	Percentage.
Primary for injury	172	27	15.69
Secondary for injury	46	2	3.34
Pathological	55	9	16.36
Total	273	38	13.91

Arm and Shoulder.

	Cases.	Deaths.	Percentage.
Primary for injury	107	13	12.15 nearly.
Secondary for injury	22	6	27.27
Pathological	13	2	15.38
Total	142	21	14.78

Forearm.

	Cases.	Deaths.	Percentage.
Primary for injury	80	4	5.00
Secondary for injury	10	2	20.00
Pathological	10	1	10 00
Total	100	7	7.00

	Cases.	Deaths.	Percentage.
Total Primary	433	63	14.54
Total Secondary	107	16	14.95
Total Pathological	146	21	14.38
Total	686	100	14.57

The causes of death were returned in too few instances to furnish grounds for trustworthy generalizations. Among the small number reported there was a fair proportion of pyæmia and septicæmia.

Assuming these statistics to be a faithful exponent of the average results of amputation in private practice, I would observe that before they can be held to warrant deductions unfavorable to hospitals, it must be shown that in all other particulars likely to exert an influence on the death-rate, the two sets of cases were precisely parallel, and that the difference of results could thus be ascribed to nothing else than to hospital influences. This, it is needless to say, it would be impossible to do. In one particular, quite aside from any influence of the hospital as such, there must almost always be an important difference. Hospitals which obtain their inmates from the poorer classes of our cities, even though they themselves be situated in the

suburbs with extensive grounds and all other advantages of rural life, must always have a higher death-rate than is found in country hospitals and in private practice among the hardy residents of agricultural districts. It is noticeable that the difference is much less in respect to amputations for disease, where the original superiority of the country people has been, in a measure, lost under the depressing influence of wasting sickness.

Thus, while in the Massachusetts General Hospital the percentage of deaths from primary traumatic amputations was 35.46, that of pathological amputations was only half, or not quite half, namely, 17.34 per cent.; whereas in the cases collected from private practice, mostly in the country, the mortality of pathological amputations was as great as that of the traumatic cases, and nearly if not quite as great as the same class of operations in well-conducted hospitals. This confirms a deduction made by Mr. Simon from Mr. Teale's figures, which went to show that while the death-rate of the city hospitals for traumatic amputations is higher than in country hospitals as 50 to $37\frac{1}{2}$, in other amputations the *London death-rate is a shade better than the country death-rate.* "In cases of injury, the Londoner may be less able to resist shock; and the injuries among railways, in the docks, etc., are more likely to be severe than in an agricultural community. Hospital unhealthiness, if it existed, would have affected the amputations for disease."

Furthermore, the statistics collected by Bristowe and Holmes in 1862, show that in eight metropolitan hospitals out of a total of 2874, there were only 50 from pyæmia, or 17 per 1000; while in 33 rural hospitals, out of 1086 deaths there were 37 from pyæmia, or 34 per 1000, or exactly double the proportion found in the metropolitan hospitals..

In view of such facts as these it becomes sufficiently apparent that the higher general mortality of the best conducted city hospitals than that of cottage hospitals in the country, is dependent upon other causes than concentration of traumatic atmosphere. Most of the cottage hospitals whence the favorable statistics of Burdett have been derived, are occupied by patients who get admission by letters of the subscribers, and who are generally

affected with chronic and mild disorders, remaining long in the hospital, and excluding severer cases. Thus they are exempt from that "intensity of experience" which is the lot of city hospitals fulfilling appropriately their office of taking care of those who most need the benefaction, and constantly renewing the supply as the subjects of acute injuries are rapidly removed by death or by convalescence.

The facts cited in this connection by Bristowe and Holmes seem fully to justify their conclusion that if St. Bartholomew's Hospital, just as it is, could be moved to the country with, however, its supply of patients from the city, the effect, if any, would be small. "But," they say, "if the removal were to change the supply of patients, whether as to the nature of the cases or as to the class of persons admitted; if instead of a large proportion of dangerous accidents and acute diseases, the practice of the hospital were to lie chiefly among chronic invalids and convalescents; or if, instead of the worn-out victims of want and debauchery, who are the principal subjects of injury and acute diseases in town, the healthier inhabitants of the country were to become the inmates of the hospital, then there can be but little doubt that the success of treatment would be considerably increased. If the hospitals were moved they would become receptacles of chronic cases only, and depots for acute cases and bad accidents would be constantly overcrowded."

The sanitary defects of any given hospital will generally be traceable to faults of location, faults of construction, or faults of administration. The two former classes of defects may be irremediable otherwise than by an abandonment of the faulty structure and the erection of another on a better site and after an improved plan. And while the sanitary principles which should govern the selection of a site and determine plans of construction have been carefully and exhaustively studied, less attention has been paid, either in theory or in practice, to the details of administration, which, in my judgment, exert a far greater influence on the results of surgical treatment. I mean to say that the best-constructed hospital, in the healthiest site, will furnish no guarantee of salubrity if ill-administered; while incessant and

scrupulous attention to cleanliness and ventilation, with a suitable distribution of the patients, will go far to wipe out the opprobrium of hospitalism in even the largest hospitals of populous cities, the death-rate under such circumstances being as low as, if not less than it would be among the same class of persons treated in private houses.

It would not be out of place to notice, in this connection, the leading principles of hospital construction and administration; but inasmuch as such a notice would extend this paper to an inconvenient length, and there is a pretty general agreement as to the sanitary requirements in both relations, I must content myself with a very brief statement of the most essential points.

That the system of isolated pavilions, of one or more stories, offers certain advantages in respect of facilities for natural ventilation and for preventing the diffusion of a traumatic atmosphere from one ward into others, will be generally admitted; and when circumstances permit, preference ought, I think, to be given to this system. But this is often precluded by obvious considerations for the central districts of populous cities where yet hospitals of considerable capacity are indispensable to meet the wants of the laboring and pauper classes. The experience of such hospitals as St. Bartholomew's with its four stories of wards, and the Glasgow Royal Infirmary with five, seems to show that, with due precautions, one story need not share in the atmosphere of another. The intervening ceiling and floors should be impermeable, and not absorbent, and the ventilation should be from the outer air. The opinion, so often expressed, that it is dangerous to place one ward over another, is founded either upon purely theoretical considerations or upon results obtained from hospitals otherwise ill-constructed or badly administered.

The same may be said with reference to the limitation of the size of hospitals. There is no adequate proof that small hospitals, other things being equal, are more salubrious than the large. Thus no such difference is found to exist between the larger and smaller hospitals in London. Indeed, the largest hospital (St. Bartholomew's) in that city had a death-rate for the ampu-

tations of three years, 1860–62 inclusive, of only 17 per cent.,[1] while Burdett only claims a death-rate of a little over 17 per cent. for the amputations in sixty-one cottage hospitals.

It often happens that a small hospital, so situated as to create a heavy pressure upon its resources, may for that reason exhibit a high mortality in spite of an excellent sanitary condition, the result being due to the greater gravity of the cases admitted. In like manner an improvement in the death-rate may follow enlargement of accomodations which permitted the admission of a larger proportion of slight cases. Bristowe and Holmes cite striking examples of both kinds.[2]

Septic complications are, indeed, likely to arise in a ward, whether of a small or a large hospital, in which a number of surgical cases, with open, suppurating wounds, are placed in proximity to each other; but by diluting these with non-surgical cases, and thus preventing over-crowding, it is quite possible to prevent the spread of such affections by adequate ventilation and scrupulous cleanliness, using this term in its most comprehensive surgical sense, as including not only cleanliness of person and clothing, but perfect purity of the atmosphere, to be maintained by efficient ventilation, proper drainage and sewerage, and such precautions as will tend to prevent the introduction and retention of morbific germs, and effect the destruction of those which may be unavoidably present. Thus all the walls and ceilings should be as little absorbent as possible. All unnecessary furniture should be excluded, and the bedding be frequently cleansed and disinfected. No sponges should be allowed in the wards—some such arrangement as exists in Morton's ward carriage being substituted therefor. No person who has spent some time in the dead-house or in the dissecting-room, should be permitted to come into a surgical ward, much less to assist in operations or in dressing open wounds, until he has been thoroughly washed and has entirely changed his clothing. There should be a complete isolation of patients with fœtid ulcerations, especially those of a cancerous nature. Erichsen advises that not more than

[1] Bristowe and Holmes, loc. cit.
[2] Bristowe and Holmes, loc. cit.

half the patients in any ward should have suppurating wounds, nor more than one-third, if they be severe, and that all cases of septic diseases be instantly isolated. He insists, moreover, that every surgical ward should be closed once a year for a month, during which time it should be thoroughly disinfected and whitewashed. And above all, and under all circumstances, overcrowding, for however short time, must be avoided.[1]

Overcrowding he defines to be "the accumulation within one ward, or under one roof, of a greater number of patients than is compatible with such a degree of purity of air, as to render the septic poison incapable of development, or if generated, of propagation in it." It is not, therefore, a fixed standard of cubical space per head for all diseases, which determines the limits of safety in respect of overcrowding, but a standard which must vary very largely with the nature of the cases, those with open suppurating wounds requiring much more air space than unwounded cases. As just intimated, all cases of septic disease should be instantly isolated, and when the weather permits they should be allowed an indefinite amount of fresh air by open opposite windows, with as large an amount of air space as circumstances will permit.

Ventilation, and preferably, perhaps, natural ventilation, is, indeed, the essential desideratum in hospital construction and hospital management. As has been already stated, Mr. Savory ascribes the surgical success of St. Bartholomew's to this, among other sanitary influences, that ventilation is effected only by open windows and chimneys. Bristowe and Holmes, emphatically indorsed by the medical officer of the Privy Council, are equally decided in their preference of natural ventilation, asserting, indeed, that adequate ventilation has never been effected by artificial machinery, all attempts in this direction having proved costly failures. They ascribe the surgical unsuccess of the costly hospital of La Riboisiere, of Paris, to the failure to use the windows of its fine pavilions, in lieu of which (natural ventilation), very beautiful and ingenious systems of artificial ventilation are substituted. They believe that, with open fire-places, provided

[1] Hospitalism, by J. Eric Erichsen, 1874.

the cubic space is ample, the beds well apart from each other, and the windows constantly open, no accessories whatever are necessary, and they conclude that with a due supply of fresh air, almost every shape of hospital and of ward has been found to fulfil its purpose; and without it none of those which they visited, admirable as many of them were in some other respects, were found to be safe receptacles for the sick and wounded. Some examples of a low rate of surgical mortality in old hospitals constructed on the block system, when they were well managed with reference to ventilation and other sanitary requirements, have been cited. I will again allude to the Pennsylvania Hospital of this city in connection with statistics reported by Dr. Wm. Hunt in 1874.[1] He shows that of the 90 deaths occurring among 1387 surgical patients in that hospital for the year 1872–3, 63 were fatal in themselves, whether in or out of hospital, leaving a mortality of 27 to be accounted for, of whom four are put down to pyæmia. The others were made up of very severe cases, such as compound fractures, exhaustive suppurative troubles, necrosis, etc. He questions whether in 1387 surgical cases, mostly very serious, in private practice, four cases of pyæmia would not occur. He is very certain of it, if the 1387 had been treated in the places where they lived.

I may now venture to answer, with some degree of confidence, some of the questions raised in the beginning of this paper.

The largest proportion of amputations, those, namely, for injury, are subject to a source of danger which does not apply in an equal degree to ovariotomy cases. These are comparable with pathological amputations, and the death-rate of the latter in private practice and in well-administered hospitals will compare quite favorably with that of ovariotomy under the most successful operators. And as to the primary traumatic amputations, with their liability to fatal shock, there is ground to believe that the mortality can be largely reduced by postponing the operation until thorough recovery from the shock of injury, while yet inflammation is averted by carbolizing the crushed

[1] Philadelphia Medical Times, Nov. 24, 1874.

tissues, and that this may be done in hospital with as much certainty and security as in private practice.

That the death-rate of amputations in hospitals deriving their inmates from the pauper population of large cities will ever be higher than in private practice or in country hospitals, is more than probable; but there is a much more satisfactory explanation than any assumed occult influence of hospitalism. Mr. Holmes has shown by an analysis of 83 deaths after amputation, with reference to the causes of death, that in a considerable proportion, namely, 14 out of 83, death was really inevitable, though not known to be so at the time of amputation, the true causes of death being revealed by autopsies. Furthermore, that of the remaining 69, in 33, or nearly one-half, death was due to the joint effects of the operation and other causes, these other causes having a main share in the result. Now it is safe to assert that these undetected complications which have so large an agency in determining the fatal issue of amputations, are far more common among the subjects of treatment in city hospitals than among the rural populations. Deducting these two classes of cases, and comparing the mortality after amputations in private practice and in hospitals, there is no satisfactory evidence that the latter, if well managed, may not give as good results as the former. But after securing all that can be accomplished by the most scrupulous cleanliness, and after we have thereby rendered the atmosphere of a hospital comparatively *aseptic*, we can never be sure that the exclusion of septic germs is absolute and complete. On the contrary, we may be quite sure that this is always impracticable. Wherever putrefaction can occur, there open wounds are liable to the risks of putrid absorption and septicæmia.

Dr. Burdon-Sanderson, in his recently published Lumleian Lectures on Inflammation before the Royal College of Physicians, has shown, by positive experiments, that septic bacteria, or their germs, in a viable state, are not necessarily phlogogenic, and that "although neither the exudation products of a normal inflammation alone, nor septic organisms alone, are infective, the association of these two conditions is always so; in other words, that exudation fluids which are infective, owe that property to

the chemical changes which septic ferments determine in them, and conversely, that septic organisms which are infective owe their infectiveness to the exudation soil in which they are grown." "Thus the presence of septic organisms (provided they have not been bred inside the body) is not necessarily attended with danger, but they become sources of great danger when they are brought into relation with devitalized organs, or with extravasated serous liquids of any kind, and particularly with inflammatory exudation."

In further elucidation of this subject, he refers to experiments which show that after an injection of a liquid charged with bacteria, into the peritoneal cavity, the lymphatics leading from the peritoneum swarm with these organisms, which also then abound in the blood. "If the animal is killed, say ten days or a fortnight after the injection, yellowish or opaque deposits are found here and there in the internal organs, particularly in the lungs, liver, and kidneys, which have been conveyed by the blood stream to the places in which they are found. If instead of charging the circulation with harmless bacteria grown outside of the body, you produce an acute peritonitis, the effect is the same mechanically, but pathologically very different. In both cases the peritoneum contains liquid charged with septic organisms, which, in both cases, are carried by the lymphatics into the blood stream; but in one case they enter the circulation incapable of doing any harm, except such as may result from the plugging of capillary arterioles; in the other, they carry with them an infective virus by virtue of which, wherever they lodge, they become the starting points of infective abscesses."

It is worthy of note that this demonstrated acquisition of virulent properties by septic bacteria from the exudation soil in which they may be made to grow, is in beautiful accord with the more recent observations of Pasteur, Sternberg, and H. C. Wood of Philadelphia, in respect of other noxious bacterial organisms and micrococci, so that the influence of the soil in which these organisms grow, has now become a well-established principle of wide application.

It is to be hoped that the lucid exposition by Burdon-Sander-

son of principles clearly and necessarily deducible from carefully-conducted experiments will finally settle all disputes as to the rôle of septic micro-organisms in the production of septicæmia and pyæmia. On the one hand, they refute the extravagant doctrine distinctly enunciated by Prof. Hueter, that inflammation is impossible without the agency of morbific germs, and that consequently the most severe injuries can be inflicted without producing inflammation if germs be excluded; and on the other, they indicate the absurd unreasonableness of those who obstinately maintain, "in the face of all the experimental investigations relating to the subject during the last few years, that these organisms are without pathological significance." . . . "If they are present in every intensely infective inflammation, we may be quite sure that they stand in important relation to the morbid process."[1]

Now, I cannot agree with the assertion of Mr. Savory, that for the prevention or removal of either or both of the two factors in the production of the infective virus, "free ablution with clean water is the simplest, safest, and best of antiseptics." Simple, of course, it always is. Safe, it may often be, when the situation of the wound and its form admit of perfect irrigation without retention of fluid in any of its recesses; but under other circumstances, it cannot be safe, and therefore cannot be the best of antiseptics, or even a moderately good one. For Burdon-Sanderson has shown that water, whether contaminated with septic organisms or not, may be introduced into the peritoneum in limited quantity without effect. But if water, which has just been boiled and cooled (thus perfectly sterilized) is injected with antiseptic precautions in larger quantity—that is in quantity so large that it cannot be rapidly absorbed—the result is entirely different. The water accumulates in the serous cavity, becomes charged by diffusion, first, with the salts, subsequently in increasing proportion with the proteids of the blood, and thus becomes a most favorable soil for the development of septic organisms which, in their turn, act infectively on the blood-

[1] Lumleian Lectures on Inflammation, by J. Burdon-Sanderson, M.D., LL.D., F.R.S., London Lancet, April 1st, 8th, and 22d, 1882.

vessels and living tissues with which they come in contact. And thus, even Mr. Savory's "clean water," if allowed to settle in any of the recesses of a wound, unless it be promptly absorbed, will become a source of positive danger by becoming charged with the exosmosed salts and albuminoids of the blood, and thereby creating a good soil for the development of infective organisms. When, then, the form and position of the wound are such as not to admit of effectual drainage, an additional protection may be derived from the use of special antiseptics, applied in conformity with the principle now generally associated with the name of Prof. Lister; that is the destruction of all germs which may be in contact with the injured tissues and the subsequent exclusion of all others. The precise details of Lister's dressings are not necessary for the establishment of this principle. Prof. Esmarch has recently stated that, by the use of pads soaked in absolute alcohol, fixed to the wound by an iodoform bandage, with some less important accessories, he had obtained the most satisfactory results. Out of 398 major amputations, there were only six deaths, 1.5 per cent., and none of them from septic complications. Of the cures, 85 per cent. healed by the first intention with one dressing. In the other 15 per cent., the dressing was renewed. Of fifty-one major amputations, only two died; one from shock and hemorrhage, and one from delirium tremens.[1]

Mr. Wm. McCormac reported, in December, 1879, that of forty-five operations in hospital for the division of various bones, in order to remove deformity—among the most dangerous kinds of operations, especially when, as in thirty of the number, the knee-joint was engaged in the operation—every one of this considerable series recovered in the speediest, easiest, and safest manner possible, under antiseptic precautions, and in every one of the thirty in which the knee-joint was opened, a freely moveable articulation resulted. On the other hand, of fifty-four cases, during the previous six years not treated antiseptically, and from which all cases involving injury to the joints were excluded,

[1] British Medical Journal, Oct. 1, 1881.

twelve died, of which four were from pyæmia and four from erysipelas. There were also other cases of erysipelas which recovered after long illness. Volkman's experience has been precisely similar.[1]

Dr. J. Ewing Mears states that erysipelas and sloughing wounds have been entirely banished from St. Mary's Hospital in Philadelphia by rigid antiseptic methods, the hospital being situated in the midst of railroads and factories, and receiving every year a large number of severe injuries.

It has been repeatedly and offensively charged against Mr. Lister that his statistics are not published. The charge is not wholly true. Mr. Lister, in defending himself against this charge, after stating that he had had only one case of pyæmia in five years in a hospital in Edinburgh, where he had an average of sixty patients under his care, gave the following statistics:

Seventy-two cases of injuries, with four deaths, or 5.5 per cent., as against 7.47 per cent., representing the boasted success of St. Bartholomew's, as reported in Mr. Savory's address. Then, of 845 operations, there were thirty-seven deaths, or 4.39 per cent. Subtracting 120 minor operations, of which not one died, 725 major operations gave thirty-seven deaths, or 5.10 per cent., as against 5.82 per cent. at St. Bartholomew's. He added that he might have so manipulated his statistics as to include some of the fatal cases among the minor operations, as they were minor in respect to the character of the operation, such as introducing two or three horse-hairs in a spina bifida, or in a hydrocephalus, etc., which would have reduced the percentage of deaths after major operations. He further stated that he had shown over and over again that a blood-clot, no matter how large, exposed in an open wound, may, under strict antiseptic treatment, remain not only free from putrefaction, but may remain indefinitely without suppuration; so that when, in the course of time, its upper surface is peeled away, one finds a scar without a single drop of pus having been formed. This, he justly contends, is a new principle in the history of surgery. He

[1] British Medical Journal, Dec. 6, 1879.

has also shown that a disinfected mass of dead tissue may be absorbed in a way never before supposed possible, as, for example, the cat-gut ligature.[1]

More recently, Dr. Hamilton, of Edinburgh, has shown that sponge, or any porous organic substance, rendered thoroughly antiseptic, may be safely incorporated with granulation tissue, which grows into its interstices.

Mr. Watson Cheyne, Surgical Registrar, King's College Hospital, has selected as crucial tests of the antiseptic method, all the cases in Mr. Lister's hospital practice between September, 1871, and November, 1879, in which healthy joints have been opened by incision, or through accident, and kept open for some days. In making a statistical report of these cases in 1879, Mr. Cheyne announced, on behalf of the author of the antiseptic system, that the spray was the least important of the various antiseptic means, and that which could be most easily dispensed with, inasmuch as there are, undoubtedly, fewer organisms floating about in the air than are present on any instrument which has got dust or water on it. For many years Mr. Lister carried out the treatment without the spray. His readiness to abandon it need not, therefore, have created such surprise as was manifested when he replied to Mr. Keith's condemnation of this element of the antiseptic apparatus at the late International Medical Congress of London.

Mr. Cheyne describes a particular case in which the knee-joint was opened in order to remove an exostosis growing from the external condyle, as representing the "typical aseptic course" —there being a slight febrile reaction on the second day, as indicated by the temperature, about 100° F., but with no impairment of appetite, and no other sign of fever being present. There was no pain, no inflammation, no suppuration, but only a slight serous discharge which rapidly diminished in amount, and finally there was no impairment of the mobility of the joint.

Where the joint has been opened by accident and dirt or dust-laden air has entered the joint before the surgeon sees the

[1] British Medical Journal, Dec. 6, 1879.

case, the sanitary "problem is not only to prevent the further entrance of causes of putrefaction, but also to destroy the vitality of those which have already gained admission, by injections into the wound and joint of a strong antiseptic solution which must penetrate into all the recesses of the wound. Usually, if the case is seen early after the accident, a solution of carbolic acid of one to twenty will suffice, but if several hours have elapsed, a solution of the same agent in rectified spirit, one in five, will be necessary."

One of Mr. Cheyne's tables gives the details of twenty cases of operations on joints, most of which were very bad cases, and all were cured of the affection for which they were treated, except one, a case of ununited fracture of the neck of the femur of eighteen months standing, and even that was greatly improved.

Two of the cases were for the removal of loose cartilages from the knee-joint.

In one remarkable case of ostitis of the tibia, in scraping out a soft granulation material from the interior of the shaft, the gouge accidentally passed into the ankle-joint, which was healthy. Chloride of zinc was applied and a drainage-tube inserted. No constitutional or local disturbance followed the operation, and the cure was complete in three months without pain and with a freely movable ankle-joint.

The second table contained 12 cases of various degrees of injury to the bones, with opening of joints. All were cured but one, which subsequently underwent operation.[1]

Mr. Lister asserts that he has published numerous cases to show that he has opened large abscesses connected with disease of the vertebræ, and then, by the careful use of strict antiseptic treatment, he has not had another drop of pus. Dr. Stephen Smith, of New York, bears similar testimony in respect to these abscesses, which, under almost any other mode of treatment, have so long been the opprobria of operative interference. Sir

[1] British Medical Journal, Nov. 29, 1881.

Statistical Report of some of the Results of the Antiseptic Method of Treatment in Mr. Lister's own hands. By W. Watson Cheyne, F.R.C.S., Surgical Registrar King's College Hospital.

James Paget said, two years ago, that until recently he had never seen a patient recover who had a lumbar abscess opened by free incision. Every one died. "But now," he adds, "I have seen several opened with absolute impunity under antiseptic treatment." In respect of amputations and other general operations, he thinks the lowest rate of mortality yet obtained may be reached by complete care, simple dressing, perfect sanitation of hospitals and private houses (there being no decided difference between the results of his private practice and those in his wards at St. Bartholomew's, except what might be fairly assigned to the difference in the character and positions of the patients), good nursing, and the keen and constant attention of the surgeon. But there remain certain groups of cases, in which he considers the complete antiseptic treatment to be absolutely essential to success. He thinks that, without any comparison, Prof. Lister has contributed far more than any one else to the present great improvement in the mortality after operations. He only doubts whether he has done more good by antiseptic treatment, than by provoking others to do their best in other ways.

Let us, then, conclude that Listerism, in connection with amputations, if not always and everywhere necessary —if perfect sanitation, good nursing, and the keen and constant attention of the surgeon be provided—is yet the "safest" of methods as obviating occasional unavoidable defects of hygienic management, whether in hospitals or private practice.

I have collected the records of 86 cases of ovariotomy in American hospitals, giving 18 deaths—a percentage of 20.93; and 311 cases in private practice, with 95 deaths, or the large percentage of 30.54. The smallness of the total number in each class, especially of the hospital cases, and the extraordinary diversities of the conditions, as shown by a detailed analysis, go far to divest this result of any special significance.[1] And yet I am led to believe that, with such a state of hospital sanitation as should be enforced at all times, and with the isolation of this class of patients, there are certain advantages on the side of the

[1] See Appendix A.

hospital, as compared with the average conditions in private houses.

With reference to this point, the statements of Dr. Wm. Goodell, of Philadelphia, in grouping his cases, are very suggestive. Up to March 6th of the current year, this eminent gynæcologist had operated in 67 cases of completed ovariotomy, and in one case in which, owing to adhesions, no attempt was made to remove the cyst. In this case, a drainage-tube was put in, and the patient recovered. Of 67 cases, 25 were operated on in the Hospital of the University of Pennsylvania—a general hospital—with 6 deaths. Eighteen were performed at the homes of the patients, but too far away for him to attend to them afterwards; of these, 7 died, or 38.80 per cent. Thirteen were operated on at home and had his subsequent care; and of these, 4 died, or 30.76 per cent. Eleven were operated on at his private hospital, with one death. This single fatal case in his private hospital was one of malignant disease of the ovaries, with universal adhesions, and was almost necessarily fatal. This private hospital is so near that he is able to see his patients very frequently, until the period of danger is past. He states in a letter to Dr. Bigelow, of Boston, that in two cases lives were saved by reason of his proximity to the hospital.[1]

Dr. Homans, of Boston, has operated 47 times, at Carney Hospital, on Dorchester Heights, overlooking the city, the harbor, and the bay, with only 4 deaths; of which, one was from acute mania, on the eighth day. A careful autopsy showed everything healthy and going on well in the peritoneum. There was hereditary insanity in the family. Excluding this case altogether, there were 3 deaths to 46 cases performed in hospital, or the small percentage of 6.52. Including it as a fatal case, the percentage would be 8.50.

His cases in private practice, performed with antiseptic precautions, which alone could be compared with the hospital cases, all of which were antiseptically treated, were 21 in number, with

[1] Dr. Goodell's last ten successive cases of completed ovariotomies, all done antiseptically, were all successful. March 6, 1882.

2 deaths—a percentage of 9.52; a slight excess on the hospital mortality. These sixty-eight cases reported in the *Boston Medical Journal* of January 23, 1879, January 20, 1881, and January 26, 1882, include all his ovariotomy cases except his first five, which were performed without antiseptics, and all were fatal.

Mr. Spencer Wells has also shown, that in his large experience there has not been any considerable difference in the results of his hospital and private practice. In 1874 and 1877, he had only 7 deaths in 71 cases of hospital practice—rather less than 10 per cent.; a better result than in his tenth and last series of 100 cases, exclusively in private practice.

It appears, therefore, that ovariotomy cases, provided they are separated from other surgical cases, do rather better in well-regulated hospitals than in private practice.

It has just been stated that Dr. Homans, of Boston, lost five cases in succession, the same having been treated without antiseptics, and that after adopting antiseptic precautions he has operated sixty-eight times, with six deaths—a percentage of 8.82, or, excluding the case of acute mania, only 7.46 per cent.

The experience of Spencer Wells is also decidedly in favor of the antiseptic treatment. He states that long before Mr. Lister had tried any of his methods, he had himself insisted upon all possible care in protecting patients before, during, and after the operation, from all the known causes of excessive mortality, and had taken unusual precautions against any risk of any contagious or infectious disease being communicated to a patient, and against the entrance from without or the development from within, of anything which could set up traumatic fever or blood-poisoning. Obstetrical cases, cancer, offensive discharges were excluded from the same building, etc. "But in no series," he adds, "had I succeeded in reducing the mortality below 17 per cent. Then in the last series of 100 cases, all done antiseptically, the mortality fell to 11. Indeed, of 113 cases, since adopting more complete antiseptic precautions, there were only 12 deaths, or 10.6 per cent."[1]

[1] Spencer Wells. Two hundred additional cases of ovariotomy, making 1000 in all. Med. Chir. Trans., LXIV., 1881.

He is still doubtful about the spray. In prolonged operations the chilling effect upon patients may be injurious. He had never seen carbolic acid poisoning. He finds a great gain from the antiseptic method is that drainage of the peritoneal cavity is now scarcely ever necessary. He has not drained a single case in which antiseptic precautions had been taken. Under proper antiseptic precautions, no putrefaction takes place and absorption is harmless.

In like manner, Mr. Knowsley Thornton, surgeon to the Samaritan Free Hospital, which had been the theatre of many of Spencer Wells' triumphs, avows his adhesion to pure Listerism, to the exclusion of all so-called modified Listerism. He is convinced that the bad results sometimes ascribed to Listerism, should more properly be ascribed to imperfect knowledge of his teaching, and consequent imperfect application of his method. It is, he avers, by rigid adherence to the teachings of Lister that he has succeeded in lowering his rate of mortality in ovariotomy from 23.94 per cent. to 4 per cent.

In his first set of cases, which were non-antiseptic and extra-peritoneal, the mortality was .	23.94	per cent.
Second set, non-antiseptic and intra-peritoneal	19.35	"
150 antiseptic cases, all intra-peritoneal . .	10	"
First 75 of these antiseptic cases . . .	16	"
Second 75 of these antiseptic cases . . .	4	"

An interesting table, added by Mr. Thornton, throws much light upon the various conditions which influence the results of this operation. Thus, of the 150 complete ovariotomies treated antiseptically,

50, with no adhesions, all recovered.
30, with slight adhesions, all recovered.
70, with extensive adhesions, gave all the deaths, 15 in number.
15 in private houses, all recovered.
6 in nursing houses, all recovered.

Note, however, that a large majority of the 21 cases constituting these last two groups were free from extensive adhesions, only four or five being noted as having such adhesions.

50 had been tapped and 7 died . . .	14 per cent.
100 had never been tapped and 8 died . .	8 "
110 had 1 ovary removed with 10 deaths . .	8.54 "
33 had both ovaries removed with 5 deaths .	15.15 "
12 cases of drainage with 3 deaths . .	25 "
6 cases completely drained with 2 deaths .	33.33 "
6 cases partially drained with 1 death . .	16.66 "

Among the fifteen deaths, three were from undoubted septicæmia, but Mr. Thornton is convinced that the septicæmia was due to previous non-antiseptic tapping.

It is noticeable that he ascribes the occurrence of acute pleurisy, in two fatal cases, to chilling from the spray in very cold weather, thus realizing an apprehension entertained by Spencer Wells.

Mr. Thornton's experience leads him to these conclusions:—

"1. In the simple cases the patients recover under Lister's method with a certainty previously unknown.

"2. There is less fever, and convalescence is more rapid than under the old method.

"3. The success obtained in the more complicated cases is in proportion to the exactness with which the antiseptic method can be applied to the individual cases.

"4. The accidents and complications occasionally following operations, such as hemorrhage, for example, are more easily overcome in cases which are strictly aseptic.

"5. There are difficulties, and even dangers, in the application of the method, and the more experience the individual has in it the more readily he foresees and avoids them, and the more complete becomes his success in applying it."[1]

On the other hand, Dr. Granville Bantock, another surgeon to the Samaritan Hospital, in a paper on "Hyperpyrexia after Listerism," refers to the danger of carbolic acid poisoning, of which children are particularly susceptible, and states that he has gradually diluted his spray and solutions so as to reduce the whole question to one of cleanliness, which he thinks is, after all, the secret and merit of Listerism.

[1] Med. Chirurg. Transactions, LXIV., 1881.

In his non-Listerian cases, 36 in number, there were 9 deaths.
" pure " " 36 " " " 8 "
" modified " " 90 " " " 8 "

In the last there was not one death from septicæmia; whereas among his pure Listerian cases there was one death from septicæmia and one from carbolic acid nephritis.

All the world has been informed with every kind and degree of sensational statement that Mr. Keith electrified the surgical section of the late International Congress by informing his hearers, that, though he had at one time a series of eighty recoveries under the spray, he had reluctantly given it up, believing that, on the whole, it did more harm than good. He had had two deaths in hospital patients, occasioned, he believed, by carbolic acid poisoning. Accordingly, he had not used the spray in his last twenty-seven cases, all of which had recovered readily. It will be observed, however, that this applies only to the spray, in regard to which Mr. Watson Cheyne, speaking for Prof. Lister, two years before, had already expressed doubts.[1]

Mr. Lawson Tait, giving his "Account of 110 consecutive cases of abdominal section performed since November 1, 1880," states that they were all performed without antiseptics, and with a mortality of only 9, or a percentage of 8.18; and that in the operations for strictly ovarian cysts (51 cases) there were but two deaths, or only 3.92 per cent. Regarding this as an immense advance on anything hitherto obtained, he takes to himself "some credit for having burst one of the largest, most blown, and most attractive bubbles ever displayed to a surgical audience."[2]

On a previous occasion this gentleman had expressed the opinion that the progressive lessening of the death-rate in ova-

[1] Mr. Keith had previously, in an elaborate paper "On Ovariotomy Before and After Antiseptics" (British Medical Journal, October 19, 1878), expressed the opinion that by antiseptics we had made the following gains:—
(1.) It had lessened mortality.
(2.) Earlier operations may be safely recommended.
(3.) Drainage might in a measure be superseded.
(4.) Convalescence was rendered easier.
(5.) It had brought great comfort and relief to the operator.

[2] Medical Times and Gazette, November 5th and 26th, 1881.

riotomy indicates nothing more than mere increase of general skill. In his first fifty cases he had nineteen deaths and in his second fifty only three. He attributed the great success of his own practice and that of Mr. Keith, to the systematic adoption of the intra-peritoneal method of dealing with the pedicle, and asserted that Mr. Spencer Wells resorted to this method nearly concurrently with his use of Listerian measures.

Dr. Walter F. Atlee, of this city, who has operated with success in St. Joseph's Hospital and in private practice, never has used Listerism. He is careful not to allow anything to flow from the tumor into the abdominal cavity, and does not permit any fingering or sponging of that cavity.

His father, Dr. John L. Atlee, of Lancaster, who had a large experience and most excellent success, has only practised complete Listerism in a single case which was successful; but for many years he has used weak solutions of carbolic acid in keeping the hands, instruments, and sponges clean.[1]

Dr. H. R. Storer, in laying down his "Golden Rules" for ovariotomy, published in the *Boston Gynæcological Journal* for December, 1869, does not include Listerism, but attaches importance to several weeks of prophylaxis, and, after the operation, to "a free diet, an appeal to skin and kidneys, patience and careful watching;" which he says will be sure to win, provided the surgeon does not vaccinate the woman with erysipelas from another patient, or with septicæmia by tying little sloughs into the peritoneal cavity, or allowing clots to remain and putrefy therein.

Such facts cannot be regarded as without significance. They show that the highest success has been reached without the use of the Listerian dressings, but they do not convince the majority of skilled ovariotomists that it is safe to dispense with the additional security which the Listerian method confers. It is the part of wisdom to use every possible precaution against the development of septic conditions. That the Listerian precautions do avail to this end, the experience of Spencer Wells, Dr. Goodell,

[1] See Appendix B.

and Dr. Homans abundantly proves. Operating under identical conditions in all other respects, they found a notable difference in the results of their antiseptic and non-antiseptic cases. In view of such experiences on the part of such men, we cannot be misled by Mr. Tait's dogmatic declaration that the terms "septicæmia" and "septic peritonitis," which appeared in the mortality columns as the explanation of the deaths after ovariotomy, are simply nonsense and have led surgeons astray altogether.

Postscriptum.

In the discussion which followed the reading of the foregoing paper, the writer was astonished to hear one or two eminent surgeons utter the senseless cry that "Listerism is dead." Some excuse might be found for the impulse which found expression in such a phrase, when Mr. Keith startled the Surgical Section of the late International Medical Congress of London by the wholly unexpected recantation of his faith in the spray, and Mr. Lister seemed for the moment to be about to abandon it. But that such a conclusion should be deliberately pronounced ten months afterwards, in the face of the fact that the practice is upheld by such men as Paget, Spencer Wells, Knowsley Thornton, and many others in England, and by numerous surgeons of equal eminence in Germany, France, and America, is simply preposterous. Those who assert it probably intend to say, that *in their opinion* Listerism ought to die. As a matter of opinion this is intelligible, and will doubtless be received with as much consideration as may properly be due to the opportunities which the parties concerned may have had for making a fair comparison of Listerism with other methods. It deserves notice, however, that every person who denounced Listerism in the discussion in Philadelphia, took credit to himself for never having tried it.

Since the date of that discussion, the testimony from distinguished sources in favor of the antiseptic method has been largely augmented. I note particularly the statement made by the editor of the *British Medical Journal* (number for May 20, 1882) as to the "comparative observations made under similar

conditions with and without spray" proposed by Mr. Spencer Wells and carried out in the Samaritan Hospital under singularly favorable conditions. The editor says: "We have been informed that the committee has expressed a strong opinion against the performance of ovariotomy for the future without full antiseptic precautions;" and he justly concludes that "if in the same institution, at the same time, by operators of equal experience, the same operation with antiseptics is followed by a mortality of about 7 per cent., and without antiseptics by one of about 30 per cent., the fact deserves very serious consideration."

I may also refer to the convincing facts stated by Prof. William Stokes in his admirable Address in Surgery at the Semi-Centennial Meeting of the British Medical Association at Worcester, England, in August of this year. After citing numerous more or less conclusive observations of his own, Mr. Stokes adds: "Similar testimony to what I and my colleagues can state has been given by many foreign surgeons of eminence, among whom I may mention von Nussbaum, Bardeleben, Esmarch, Thiersch, von Langenbeck, Volkmann, Saxtorpf, Champonnière, and many others."

And yet, forsooth, "Listerism is dead"!!

APPENDIX A.

OVARIOTOMY IN PRIVATE PRACTICE.

Operator.	No. of cases.	Age of patient.	Antiseptic precautions.	Result.	Cause of death. Remarks.
W. A. Byrd, Quincy, Ill.	1	23	Antiseptics used	Recovery	Tumor weighed 66¾ lbs.
	2	23	" "	Death	Death on 3d day from shock: tumor weighed 90 lbs.
	3	35	" "	"	Death on 3d day from septicæmia and delirium tremens: the patient for six years having drunk half a gallon of gin or whiskey daily.
John Homans, Boston, Mass.	4	38	" "	"	The tumor weighed 57 lbs.
	5—8	..	None	4 deaths	Death resulted from septicæmia in one week. Reported by Dr. G. S. Osborne. These operations were done prior to 1879.
E. D. Ferguson, Troy, N. Y.	9	30	"	Death	14 lbs. of fluid removed.
	10	35	"	"	50 lbs. of fluid removed: part of sac left and stitched to the edges of wound. Drainage and washing.
D. W. Yandell, Louisville, Ky.	11	42	Antiseptics used	Death	Death on fifth day from peritonitis. Bad adhesions.
	12	64	" "	"	Death on fourth day from septicæmia. Bad adhesions.
	13	22	" "	..	Death from peritonitis; a sponge having been left in the abdomen.
	14	34	None	Recovery	
	15	23	Antiseptics used	"	
	16	21	" "	"	
	17	21	None	Death	Death from shock; adhesion universal.
H. D. Marcy, Boston, Mass.	18	28	Antiseptics used	Recovery	
	19	35	" "	"	Both ovaries and uterus removed for fibroid tumor.
H. Culbertson, Zanesville, O.	20	(50?)	" "	Death	Multilocular tumor weighing 94 lbs. Death from shock. Operation shared by Dr. C. C. Hildreth of Zanesville. Adhesions extensive.
	21	(30?)	" "	Recovery	Monocystic tumor. Peritonitis resulted, which was subdued by opium and application of ice-water in rubber tubing to abdomen. Operation shared by Dr. L. M. Beamy of Zanesville.
J. F. Heustis, Mobile, Ala.	22	47	None	Death	Tumor very large; multilocular. Intestinal adhesion. Death from shock.
	23	27	"	"	Tumor malignant. Death from peritonitis.
	24	..	"	Recovery	Slight anterior adhesions.
	25	10	"	"	Slight posterior adhesions, with suppurating cyst.
H. McGuire, Richmond, Va.	26—34	19—48	Antiseptics used	Recoveries	One died 13 months after operation from gastro-intestinal fever, and one 5 months after operation from cancer.
	35	21	No antiseptics	Recovery	
	36	33	" "	Death	On 7th day from peritonitis and bronchitis.
James Guild, Tuscaloosa, Ala.	37	21	" "	Recovery	Tumor solid, and weighed 12 lbs.
	38	35	" "	Death	From hemorrhage on operating table. Tumor weighed over 40 lbs.

OVARIOTOMY IN PRIVATE PRACTICE.—*Continued.*

Operator.	No. of cases.	Age of patient.	Antiseptic precautions.	Result.	Cause of death. Remarks.
J. T. Hodgen, St. Louis, Mo.	39[1]	45	No antiseptics	Death	From shock and exhaustion two days after operation; sac incised and left.
	40	26	" "	Recovery	
	41	27	" "	Death	From shock and exhaustion 20 days after operation.
	42	48	" "	"	From shock and exhaustion 2 days after operation. Universal adhesions.
	43	50	" "	"	From inflammation 3 days after operation.
	44	30	" "	"	8 days after operation.
	45	47	" "	"	From exhaustion 36 hours after operation. Malignant tumor, not ovarian.
	46	48	Antiseptics used	"	From inflammation in 4 days after operation.
	47	54	" "	"	From septicæmia in 5 days after operation.
	48	17	" "	"	From septicæmia in 5 days after operation.
	49	35	" "	"	From exhaustion in 2 days after operation. Extensive adhesions anteriorly.
	50	60	" "	"	From exhaustion and shock in 2 days after operation.
	51	40	" "	"	From inflammation in 6 days after operation.
	52	17	" "	"	From septicæmia in 4 days after operation.
	53	28	" "	Recovery	
	54	47	" "	Death	From septicæmia in 4 days after operation. Malignant tumor, not ovarian.
	55	40	" "	Recovery	
	56	45	" "	Death	From septicæmia in 5 days after operation. Bladder adherent up to umbilicus.
	57	55	" "	"	From tetanus in 12 days after operation.
	58	29	" "	"	Cause not stated.
	59	20	" "	"	From septicæmia in 4 days after operation. Cystic degeneration of left kidney mistaken for ovarian tumor.
	60	46	" "	Recovery	
J. R. Weist, Richmond, Ind.	61	26	Lister's without the spray	Death	Shock in 15 hours. A suppurating compound cyst with extensive adhesions, weighed 35 lbs.
	62	52	" "	Recovery	Compound cyst weighing 45 lbs.
	63	38	" "	"	Recovered from the operation, but died from pleuritis in 15 days.
	64	35	" "	"	Simple cyst weighing 26 lbs.
	65	33	Antiseptics used	Death	From chronic peritonitis 3 weeks after operation.
	66	60	" "	"	Shock and exhaustion on 6th day.
	67	19	" "	"	Shock 3d day; sarcoma of ovary.
	68	45	" "	"	From overdose of opium on 8th day.
	69	18	" "	Recovery	
Wm. Varian, Titusville, Pa	70	22	" "	"	The total number of Dr. Varian's cases is 35, with 4 deaths. Of these 5 were laparotomies for uterine fibroids, all of which were successful. All his operations were in the country, or small country towns with a population not exceeding 5000. No detailed records of 21 of the cases.
	71	22	" "	"	
	72	23	" "	"	
	73	47	" "	"	
	74	42	" "	"	
	75	37	" "	"	
	76	48	" "	"	
	77	37	" "	"	
	78	40	" "	"	
	79—99	..	" "	"	

[1] In cases 39, 42, 45, 49, 54, 56, and 59, the operation was not completed.

OVARIOTOMY IN PRIVATE PRACTICE.—*Continued.*

Operator.	No. of cases.	Age of patient.	Antiseptic precautions.	Result.	Cause of death. Remarks.
Edward Miller, Louisville, Ky.	100	46	Washing the wound with carbolized water	Recovery	
	101	34	" "	"	
A. W. Johnstone,	102	27	Antiseptics used	Death	Death from tetanus on 10th day.
	103	35	" "	Recovery	
	104	55	" "	Death	Died on 3d day from exhaustion Cyst had been ruptured, and there had been chronic peritonitis and septicæmia for weeks previous to operation.
E. P. Bennett, Danbury, Ct.	105	23	None	Recovery	
	106	35	"	"	
	107	25	"	"	
	108	28	"	"	
	109	45	"	"	
	110	37	"	"	
	111	73	"	"	
	112	44	"	"	
	113	39	"	"	
	114	42	"	Death	Death was caused by escape of ligatures into the abdomen.
J. M. Holloway, Louisville, Ky.	115	30	Antiseptics used	"	Death was caused by exhaustive diarrhœa supervening upon septicæmia on 23d day. Paracentesis had been done 18 times in 2 years before the operation.
	116	45	None	....	
	117	30	Antiseptics used	Recovery	
	118	24	" "	Death	Death on 6th day from septicæmia accompanied by obstinate vomiting, which detached the ligatures of the pedicle.
E. M. Miller, Florence, S. C.	119	48	" "	Recovery	24 days after operation the case was doing well.
F. K. Paddock, Pittsfield, Mass.	120	30	None	Death	Death 56 hours after operation from nervous shock, vomiting having been incessant from time of operation.
	121	52	Antiseptics used	"	Death on 5th day from peritonitis.
	122	50	None	"	Death on 6th day from septicæmia.
V. P. R., Oshkosh, Wis.	123	51	Antiseptics used	"	Death in 2 hours from shock, rupture of the sac having occurred.
	124	19	None	Recovery	
	125	26	"	"	
	126	42	"	"	
	127	42	Antiseptics used	"	
	128	51	" "	"	
E. B. Turnipseed, Columbia, S. C.	129	38	Antiseptics used	"	The tumor was not removed, owing to extensive adhesions.
F. D. Cunningham Richmond, Va.	130	31	" "	"	Both ovaries removed.
B. W. Allen, Wheeling, W. V.	131	30	" "	"	Death ensued 18 months after operation from wound of cicatrix produced by a broken steel splint in corset during a scuffle with a friend.
	132	22	" "	Death	Death on 5th day from shock and exhaustion. Tumor colloid, multilocular, and adherent.
T. R. B. Boutecou Troy, N. Y.	133	30	" "	"	Death in a week from peritonitis and septicæmia. Tumor unilocular.
	134	55	" "	Recovery	Tumor multilocular, ruptured through vagina, and treated by drainage tube, and injection of tinct. iodine, and subsequently carbolized water.
Geo. H. Bixby, Boston, Mass.	135	45	None	"	
	136	32	"	"	
	137	45	"	Death	Death on 6th day from septicæmia.
	138	35	"	"	Death on 7th day from peritonitis
	139	45	"	"	Death on 5th day from peritonitis.
James E. Reeves	140	29	"	Recovery	

OVARIOTOMY IN PRIVATE PRACTICE.—*Continued.*

Operator.	No. of cases.	Age of patient.	Antiseptic precautions.	Result.	Cause of death. Remarks.
S. M. Ross, Altoona, Pa.	141	49	None	Death	On 9th day from exhaustion. Highest temp. 100°; highest pulse 85. Could retain no food. Inveterate smoker.
T. A. Harris, Parkersburg, W. Va.	142	45	"	"	On 5th day from exhaustion.
	143	40	"	Recovery	
	144	54	"	Death	From blood-poisoning on 5th day.
Dr. Hendree, Selma, Ala.	145	40	"	Recovery	
T. H. Gage, Worcester, Mass.	146	50	"	"	
	147	61	"	Death	In 30 hours from shock.
	148	42	"	Recovery	
	149	37	"	"	
	150	64	"	Death	From peritonitis, 3d day.
	151	53	"	"	From peritonitis, 4th day.
	152	52	"	Recovery	
T. G. Morton, Philadelphia.	153	28	"	"	In country.
	154	30	"	Death	From peritonitis and exhaustion. Performed in the city.
	155	15	Antiseptics used	Recovery	
S. Eagan, Dallas, Texas.	156	28	" "	"	
	157	43	No antiseptics	Death	From acute general peritonitis on 5th day. Tumor mostly solid, weighing 20 lbs.; numerous adhesions; incision 10 inches long.
H. P. C. Wilson, Baltimore.	158	..	Antiseptics	"	From cancer of both ovaries, with cystic degeneration.
	159	..	"	"	From shock in 12 hours.
	160 to 166	..	"	Recoveries	Operation in upper room, sunlight; perfect cleanliness.
J. Ewing Mears, Philadelphia.	167	18 to 64	No antiseptics.	Death	From septicæmia.
	168		" "	"	Malignant disease. Exhaustion & shock.
	169		Antiseptics	"	Septicæmia.
	170 to 183		Antiseptics in all but four	Recoveries	All but two in the city.
John L. Atlee, Lancaster Pa.	184	..	Has only used complete Listerism once, but always used antiseptics, chiefly weak solutions of carbolic acid.	Death	On 11th day from inflammation of lungs, caught by exposure to severe cold.
	185	..		"	From peritonitis on 10th day; bad nursing and over-feeding.
	186	..		"	Shock. Patient affected with consumption.
	187	..		"	Shock on 3d day.
	188	..		"	Encephaloid.
	189	..		"	Encephaloid.
	190	..		"	On 8th day from internal strangulation of colon by false membrane
	191	..		"	Inveterate diarrhœa; tuberculosis of peritoneum. Opium habit.
	192 to 236	..		Recoveries	
	237	..		Death	Shock and hemorrhage: In 5 of these cases (237–241), owing to extensive adhesions the sac was not removed, but after tapping, the abdominal wound was closed.
	238 to 241	..		Recoveries	
Isaiah White, Richmond, Va.	242	37	Drainage tube in Douglas' cul de sac	Recovery	
	243	50	Listerism	Death	From shock in a few hours.
	244	60	"	"	From hemorrhage on 3d day.
	245	38	None	Recovery	
Geo. Cupples, San Antonio, Texas.	246	47	"	Death	From peritonitis on 5th day.
	247	7 yrs. 8 mo.	Pad of lint saturated with carbolic oil 1 to 10	Recovery	
	248	36		Death	From hemorrhage on 5th day.

OVARIOTOMY IN PRIVATE PRACTICE.—*Concluded.*

Operator.	No. of cases.	Age of patient.	Antiseptic precautions.	Result.	Cause of death. Remarks.
John Homans, Boston.	249 to 251	..	Listerism	1 death	From shock in 14 hours. "Extensive adhesions." See "Cases of Antiseptic Ovariotomy, by John Homans," Boston Med. and Surg. Journ., Jan. 23, 1879. Cases 6, 7, and 8.
	252 to 258	..	"	1 death	Exhaustion on 5th day. Case 16 in Bost Med. and Surg. Journ., Jan. 20, 1881.
	259 to 269	..	"	Recoveries	Boston Med. and Surg. Journ., Jan. 26, 1882.
Wm. Goodell, Philadelphia.	270 to 287	..	"	7 deaths	Operation at patients' homes, too far for Dr. G. to attend after operation.
	288 to 300	..	"	4 deaths	Operation at patients' homes, who, however, had his care after operation.
	301 to 311	..	"	1 death	At Dr. G.'s private hospital. This was a malignant disease of both ovaries, with universal adhesions — necessarily fatal. For causes of death in other cases see detailed report in American Journal of Obstetrics for April, 1882.
	Cases, 311.		Deaths, 95.		Percentage, 30.54.

OVARIOTOMY IN HOSPITAL.

Operator.	No. of cases.	Age of patient.	Antiseptic precautions.	Result.	Cause of death. Remarks.
Walter F. Atlee	3	..	None	Recovery	
	4	..	Laparotomy for tumor of uterus supposed to be ovarian	"	Has never used Listerism. Admits but few assistants; ties the patient on a patent ironing table. Is very careful not to allow anything to flow from the tumor into the peritoneal cavity, and does not allow any fingering or sponging of that cavity.
	5	..	Laparotomy for extra-uterine fœtus	"	
Hunter McGuire	6	52	None	Death	Death 3 hours after operation from shock
	7	49	Antiseptics used	"	Death from peritonitis.
	8	50	" "	Recovery	
	9	50	" "	Death	Death from peritonitis on 5th day.
	10	54	" "	"	Death from septicæmia.
	11	21	" "	"	Death on 2d day from exhaustion.
	12	25	" "	"	Death on 2d day from " and shock.
G. B. Johnston, Richmond, Va.	13	26	" "	"	Death on 11th day from exhaustion.
	14	44	" "	"	Death from pyæmia.
Geo. H. Bixby, Boston, Mass.	15	57	" "	Recovery	Ward isolated from main building (Carney Hospital?).
	16	28	" "	"	
John Homans, Boston.	17—21	..	Listerism	Recoveries	Boston Med. and Surg. Journ., Jan. 23, 1879.
	22—39	..	"	1 death	Shock. Boston Med. and Surg. Journ., Jan. 20, 1881.
	40—63	..	"	3 deaths	One from exhaustion on 3d day; tumor vascular and nearly solid. One from acute mania on 8th day; mania hereditary. One from commencing decay of double tumor, one on each side. See Boston Med. and Surg. Journ., Jan. 26, 1882.
Wm. Goodell, Philadelphia.	64—86	..	"	6 deaths	See Am. Journ. Obstetrics, April, 1882. Performed in Hosp. of Univer. of Pa.
	87—88	..	"	Recoveries	Performed in Hosp. Univer. of Pa.
	Cases, 86.[1]		Deaths, 18.		Percentage, 20.93.

[1] The obvious error in numbering the cases of Ovariotomy in Hospital is corrected in the summary.

APPENDIX B.

LANCASTER, PA., March 13, 1882.

JAMES L. CABELL, M.D.

My dear Doctor: I have gone over very carefully my list of ovariotomies, and I send you the result.

I have operated in fifty-seven (57) cases; of these, forty-five (45) were entirely successful; three of them being double ovariotomies; all of which latter are now living. My very first case was a double ovariotomy, June 29, 1843; the first case on record. The patient is still living; and the case is reported in *The American Journal of the Medical Sciences* in Jan. 7, 1844. I send you her photograph taken in 1881.

Eight (8) of the operations proved fatal after the removal: one from inflammation of the lungs on the 11th day, from exposure to severe cold; one from peritonitis, on the 10th day, from bad nursing and great overfeeding; one from shock in a patient affected with phthisis pulmonalis, who obstinately demanded the operation; another from shock on the 3d day; two from malignant tumors (encephaloma) within three days; one from obstruction of the bowels, after progressing finely, on the 8th day; a knuckle of the transverse colon was found bound down under a false membrane arising from the omentum; one from an inveterate diarrhœa, and extensive tuberculosis of the mural and intestinal peritoneum; the patient having come from a distance, and keeping me in ignorance that she had been kept for years constantly under the influence of very large doses of morphia.

In five (5) cases, owing to very extensive adhesions to vital organs, I could not remove the cyst; and, after tapping, had to close the abdomen. Of these, one died from shock and hemorrhage, the others recovered.

I have practised complete Listerism, with spray, in but one case, which was successful; but I have used antiseptics, chiefly weak solutions of carbolic acid, in keeping the hands, instruments, and sponges clean, for many years.

The great secret of success is in having the patient in pure air,

proper preliminary treatment, in perfect cleanliness, careful manipulation, and in removing all escaping fluids, whether blood or cystic contents, by soft sponges rapidly cleansed in the antiseptic water before re-introduction; by securing all weeping bloodvessels; and where this cannot be immediately done, by using the glass drainage-tube at the time of closing the abdomen. Great danger exists from confining these fluids in the abdomen.

In the preliminary treatment I allow the patient a light breakfast about 8 o'clock A. M., the day before the operation. At about 11 o'clock A. M. I give a dose of castor-oil, and repeat it, *pro re nata*, until the bowels are thoroughly evacuated; giving nothing but simple water or barley-water after the breakfast until after the operation. As soon as the bowels are thoroughly cleansed, I "put them in splints" by small and repeated doses of opium. This is a great advantage, and in nine cases out of ten prevents their extrusion during the operation.

I generally use a mixture of one part of chloroform to three of Squibb's ether, as the anæsthetic at first, and afterwards pure chloroform, if the effect is not satisfactory; and I have never had unfavorable results.

For one or two days after the operation, I keep my patients on pellets of ice, and control nausea and pain by ice and opium; and I gradually allow very light and easily digested nourishment as the patient's stomach will bear it.

I have for many years used the clamp to secure the pedicle, believing that by leaving no foreign substance within the abdomen, there is less risk of peritonitis. I believe that in England the clamp has given way to the silk ligature cut off close, and dropping the pedicle; but my success with the clamp induces me to prefer it. Where portions of the omentum adhere to the cyst, I separate them with my hand, tie the bleeding portion with fine silk ligatures, cut it off close, and secure the end in the abdominal wound. In a few cases I have cut off the omentum close to the ligature and dropped it into the abdomen, with good results.

In the vitiated atmosphere of many Foreign and American Hospitals complete Listerism with the spray may be advisable; but Dr. Kieth, of Edinburgh, the most successful operator now living, has now relinquished the use of it, and says that two of his patients were poisoned by carbolic acid.

I believe that with proper care in every other respect, we can dispense with the *spray*.

I have now, my dear Doctor, answered, as far as I understand it, the purport of your letter. If I have made any omissions, it will afford me pleasure to supply them.

Very respectfully yours,
JNO. L. ATLEE.

LANCASTER, March 20, 1882.

With respect to my success previous to the introduction of Listerism; and indeed I may say, of antiseptics, I think it was quite as great as it has been since. Some of the cases were so desperate, that the only possible chance of prolonging life was in the operation, which I could not withhold from them; and some of them made a good recovery. My brother Washington had a similar experience.

The course I have pursued has been that mentioned in my former letter, and where that is faithfully carried out, I think it is all that is necessary.

I am, my dear Dr., faithfully yours,
JNO. L. ATLEE.

J. L. CABELL, M.D.

DISCUSSION.

Dr. D. W. YANDELL, of Louisville, Kentucky.

I think it well to suggest to my learned friend that Dr. Keith, since he made his report to the International Medical Congress in regard to the twenty-seven cases operated on without the spray, has operated on forty other cases without a death. Some of these were what he calls "heavy cases;" a few of them "very heavy cases." While he has abandoned the spray, it is proper to say that he still uses antiseptics in general surgery.

The things that he insists upon as being of the first importance are thorough cleansing of the abdominal cavity, and leaving it perfectly dry; where this cannot be done, he invariably secures drainage. Lawson Tait insists with equal earnestness on the very same points. They are united on drainage, while neither uses antiseptics

in any proper or Listerian sense of the word. And we all know that *en rigeur* Listerism means Listerism in all its details. Without these it is a delusion and a snare. I think it may be safely said that Mr. Keith has now done seventy-three or seventy-four ovariotomies without the spray, with a success as uniform as that which attended his operations done under the spray.

Dr. Beverely Cole, of San Francisco, California.

I can readily see that in a paper of the character of the one we have just listened to, and about which there is so much interest—and exhaustive as it may appear—that it is very difficult to free one's self from prejudice. I do not think that I could do it myself. To my mind prejudice is stamped upon every page of that paper; not that its author is disposed to be unfair, that I do not believe.

He has cited Mr. Spencer Wells; he has cited Mr. Bantock on the other side; he has cited Mr. Lister, but as Professor Yandell has said, he has not read anything that Mr. Lister has said, or if he did, I did not appreciate it. If I remember aright Mr. Lister, in closing the debate upon the paper of Mr. Savory, said: "I have never thought from the beginning that this system of mine was adapted to ovariotomy, and so much impressed was I with this belief, that, when Mr. Keith informed me of his intention to adopt it and introduce it into his practice, I strongly dissuaded him from doing so."

Further than this, Mr. Lister goes on to say, "I have anticipated that from my method great assistance will accrue to the operation of ovariotomy." He anticipated it, but has it been realized? The gentleman has regulated his statistics to sustain his theory. I do not know that I would be any more free from this than any other man. It is impossible in any one paper to give a full exposition of the whole question. I found, that, of the fifteen gentlemen who took part in this discussion at the International Congress, there were only two who came out positively in support of Listerism. These two were Spencer Wells and Mr. Thornton. Even Sims, who, as you will remember, in a little pamphlet issued two or three years ago, devoted largely to the subject of antiseptics and Listerism especially, at least confessed himself to be an advocate of this system; but what does he say now? When Spencer Wells rejected the drainage-tube, as he did in this discussion, he expressed surprise and astonishment that he should do so, and stated that Spencer Wells

would encounter many cases in which a drainage-tube would be indispensable. He even goes so far as to carry the impression that he is no longer the strenuous advocate and supporter of Listerism that he was a few years ago.

When we come to Dr. Letiévant, he goes on in his paper to say, that "I am satisfied that this theory of microscopical organisms is not the correct one." He states this fact: that in the Hospital, "Hôtel Dieu," at Lyons, to which he was called to investigate the ventilation, he found the maternity surrounded by the dissecting-rooms, the operating theatre, the chemical laboratory, and the surgical wards, and he says: "If there be anything in this theory—germs being communicated through the air—why have we never had any epidemic? The truth is there has never been an epidemic of puerperal fever or other infectious diseases incident to the puerperal state."

I am sorry that I have had to take part in this discussion, because I have prepared a *résumé* of what transpired at the Congress, but the subject was lagging, and I introduced these remarks more for the purpose of inducing discussion.

Dr. H. F. Campbell, of Augusta, Georgia.

The subject is not lagging, there is a great deal of interest taken in it, a great deal on both sides of the question. I have had an experience of forty years in surgery without carbolic acid.

I am getting old; and I am now too old, perhaps, to take up new things. I know that many younger men have taken it up; I do not know whether my friend, Professor Gross, our distinguished President, advocates it. I cannot take it up. I have operated where there was no chance of applying carbolic acid, as in cases of lithotomy. I never have metastatic abscess; I never have traumatic fever. Although I have operated on some sixty-seven cases I have not used a drop of carbolic acid in any one of them.

When Mr. Lister advocated carbolic acid, a number of physiologists and pathologists followed him in believing that suppuration was due to a particular cause which carbolic acid was said to combat. Why should it? Because microscopic germs cause suppuration, and, as carbolic acid is a germicide and kills this cause, suppuration is prevented *ab initio*. Well, that does not oblige the theory to be true. May we not be mistaken? What is the real *rationale*, the real pathology of suppuration? I really think that

there is some chance of a new explanation of it. It is not necessary that micrococci alone should produce suppuration. There is nothing better established, nor older, nothing more undeniable, than the old saying, that where there is irritation there is a flow. In a wound there is congestion, the result of irritation. Now I believe that our theory of suppuration is defective. It was all in a muddle and confusion in the beginning, and we seized with great avidity upon the idea that micro-organisms were the cause, and it may be that this has arrested our pathological investigations into the cause of suppuration, satisfying us that we have solved the problem.

I believe that what occurs in suppuration is this: A delicate sensitive part is injured, a splinter perhaps has entered, or a contusion has been received, or an incision has been made. What happens? What have we done? We have injured a number of sensory nerves which are distributed to every portion of the body. These sensitive nerves transmit influence to the spinal centre. This spinal centre is in intimate relation with the nerves controlling the bloodvessels in this wounded part of the body. We have, therefore, all the apparatus and elements of reflex irritation. A sensory nerve is wounded, and the nerve controlling the dilatation and contraction of the bloodvessels of this part has been the subject of aggression. The longer the irritation continues, the more influence is made on the bloodvessels. The increased flow of blood to the part will cause congestion, and I believe that suppuration is dependent upon the increased amount of blood in the part almost invariably.

What would be the means of relieving it? What does relieve it? What better means have we for lessening irritation than cold water as a sedative to the part? What better sedative remedies have we for preventing irritation from going on to inflammation, and inflammation to suppuration, than sedatives and opiates?

Dr. J. W. S. Gouley, of New York, N. Y.

I have always been a consistent anti-Listeric surgeon. I have now had twenty-three years of large hospital practice, and I have never yet wished to use Listerism in any sense whatever. I have never treated a single wound, after amputation, by closure of the wound after the Listeric plan. If it be true that the unfavorable symptoms which follow certain operations in certain conditions of the system

are due to the presence of bacteria in the wound or in the blood, then the Listeric plan, in my opinion, is the wrong one. It cannot succeed. It may reach the micrococci, the germs in the wound; but how is it to reach those that are in the blood and in the lymphatics? They cannot be reached by any local treatment, nor by closure of the wound. If they were in the atmosphere, I doubt very much if the spray itself would prevent their entering the wound and propagating themselves. I think Listerism is the wrong plan. By the by, the plan of which I am speaking, I have every proof is not that of Mr. Lister. I have seen the documents showing that a Frenchman, Dr. Déclat, as far back as 1863, proposed the treatment of wounds by carbolic acid and other antiseptic methods. He takes the ground that these septic symptoms are due to the presence of bacteria in the blood and lymph streams. If these bacteria exist in the system, then there must be constitutional treatment. He proposes the administration of either carbolic acid or other antiseptic internally. He is a great carbolic acid man. He wants to kill the germs that are inside (I have some doubts of that myself). He cleanses the part perfectly with carbolic acid, then introduces it into the system. He sometimes gives it by the bowel in enema. He introduces it everywhere. The hypodermic is a favorite method with him. He injects large amounts of carbolic acid; this I conceive to be very dangerous. I have observed several cases of carbolic acid poisoning, giving rise to hæmaturia and other characteristic symptoms, from this cause. One of our members, Dr. Donald MacLean, was poisoned by carbolic acid used at an ovariotomy.

While I am not a Listerist, I am, as all of you gentlemen are, a good sound antiseptist. Every surgeon must be, in the sense of keeping wounds clean, keeping the parts clean, using water or other substances. Alcohol for the past twenty years I have used with excellent effect in dressing wounds. We all use quinine; what is quinine but an antiseptic, a parasiticide? We all give tincture of the chloride of iron and other preparations of iron which are parasiticides. Therefore, we are really all of us antiseptists; no good surgeon can be otherwise than an antiseptist. I cannot conceive of any one getting good results from hermetically closing wounds after amputations and excision of tumors.

I do not think that Listerism is going to die—it *is* dead. Few surgeons will long continue to use it.

Dr. R. A. KINLOCH, of Charleston, South Carolina.

I think that some of my friends do Mr. Lister and his method this much injustice, that they talk simply about closing wounds, when we know that drainage is a part of the practice insisted upon by Mr. Lister. The statement of my friend in regard to the use of carbolic acid in the operation of lithotomy is, I think, rather far-fetched. Mr. Lister does not confine his surgery to carbolic acid alone; but when we come to this matter of carbolic acid, what do we find? We find that if there be any objection to it, it is the use of the spray. It is said to have poisoned patients; in how many cases? A distinguished surgeon of Edinburgh thinks that he has poisoned two patients during ovariotomy, therefore (I think not very wisely, and upon very small data) he drops the spray. If carbolic acid does no good, how many physicians are ready to contend that it does harm? How many are ready to contend that, since Listerism has been carried on, the mortality, as a whole, has been greater? The mortality has surely been lessened. The very gentleman, Mr. Keith, who excludes the spray from ovariotomy, insists upon its use (so says one of my friends here) in other procedures. Our friend tells us that Mr. Keith uses it when operating on the joints. This is very queer. There may not be as much danger from putting the spray into a joint as into the abdominal cavity; but if the spray does no good, why does he use it? I think, therefore, Sir, that we cannot and should not condemn Listerism in all its aspects. It has accomplished great triumphs. There are very few surgeons who will dare to say that the mortality has been increased by Listerism. If it is yielded that mortality has not been increased, I am prepared to prove, I think, that it has been lessened. Listerism is *not* dead; I believe that we are yet to hear more of it.

Dr. C. B. NANCREDE, of Philadelphia, Pennsylvania.

I have a few remarks to make on this subject, and I shall confine myself entirely, as most of the gentlemen have done, to the question of so-called antiseptic surgery. It is with a great deal of diffidence that I rise in such a distinguished body of surgeons; but I feel confident that I possess one qualification, which several of the speakers have lacked, *i. e.*, personal experience with the system; I can hardly think that any gentleman who has really and faithfully tried it would apply to it the remarks which some of the gentlemen

have made. In the first place, I have heard a good deal—I don't know what to term it—rhetoric, but few facts. Mr. Lister is made the butt of all the ridicule, and by his results everything is considered as either standing or falling. But Mr. Lister has, comparatively speaking, done less antiseptic surgery than others, many of whom have had an experience of three to one, and who speak in positive terms in his favor.

Many of the gentlemen have said, that, as the antiseptic system is dependent upon the germ theory, and that the germ theory is untrue; therefore, they deny the results of antiseptic surgery. Because the older philosophers incorrectly taught that the sun moved around the earth, did the *fact* of the earth moving around the sun cease to be true?

Nussbaum states that, after having tried every other means of treating wounds, he adopted strictly the Listerian methods, and the result was an immediate lowering of his mortality. What proves that it was due to the method of dressing was that, where he could not apply these methods, he had the same bad results.

The charge is also made, that it is a dangerous method, because carbolic acid is a poison. But the Listerian methods and Listerism are not the same. Many harmless antiseptics may be used instead of carbolic acid. Eucalyptus is a non-poisonous substance; it is a good antiseptic.

Again, because a man gives up the spray, the cry should not go up that he has given up Listerism. Listerism aims at depriving wound fluids of the power of putrefaction. Whether this be due to germs or something else I cannot say; but, if by certain methods of dressing you get uniformly good results under the worst circumstances (it has never been said that with the best opportunities you cannot get almost identical results), that method should be continued. It makes little difference to us whether you destroy the germ, or whatever it is, before or after it has reached the wound. If you wash out the wound with chloride of zinc, you are using Listerian principles, whether you know it or not. Lister never said that it was carbolic acid or anything else that constituted his principle of treatment. He points out that, if you have intra-traumatic irritation or tension, you will as surely have suppuration—though it may be aseptic—as though you treated it in any other way.

I wish to call attention in general terms to the actual results. Mr. Lister is generally referred to as if he were the only advocate

of aseptic surgery. Take the results of such prominent men as Bardeleben and Billroth; surely they cannot be considered as being dirty or careless; they have performed some 300 or 400 amputations with a mortality of about 29 per cent. Taking about the same number of amputations, of about the same gravity, performed by Nussbaum, Volkmann, Socin, and Lister, with the antiseptic system, the death-rate was 4.4 per cent. MacEwen, in between 500 and 600 osteotomies, has had precisely 3 deaths; one from diphtheria, one from meningitis, and the other from some cause that I do not recollect, but it was not from any septic trouble. In one of the cases that died ten osteotomies were performed by this surgeon. I don't think, because the theory has apparently failed, that we should renounce the system. I have seen a case of compound comminuted fracture, where suppuration occurred as the result of intra-traumatic tension, dressed on the eighth day, at which time there was not the slightest odor, although the accident occurred in hot weather.

They say the system of Mr. Lister is troublesome; it is not more troublesome than other methods, and if it were, we should not consider the trouble when the life or comfort of a fellow-man is in question.

I do not stand here as an enthusiastic advocate of Listerism. I tried it for several years, then I gave it up for several years, and now I am going back to it.

Dr. Geo. W. Gay, of Boston, Massachusetts.

I have had an experience of ten years as visiting surgeon in the largest hospital of Boston. To give you an idea of the work we have to do, I will tell you that we have from 1600 to 1700 surgical patients every year. About five years ago I had the honor of introducing into that hospital Lister's method of treating wounds; I have used it more or less ever since. In regard to the Listerian method of treating wounds, it is very difficult to get statistics upon it, for the slightest slip is a disturbance of the method. I mean by that, if the surgeon lays down his knife and takes up a towel and wipes his forehead, and takes up the knife again, he disturbs the method, or if his lamp goes out and his spray ceases, that is a disturbance.

About the objections to the treatment: Outside of large hospitals I do not think that the treatment will be very popular. It requires a great deal of work and care, and many physicians think it too

much trouble. It is also expensive; our superintendent has told us that some of our dressings have cost between one and two dollars each; fortunately we do not have to continue these a great while.

But people will die of septicæmia; in some, wounds will suppurate profusely, and in others erysipelas will occur in spite of the Listerian dressing. I do not think that it is going to prevent such results. After using this practice for five years in this large hospital, and seeing other cases of like character treated by other methods in the same ward (one surgeon having one side of the ward and one the other), I feel convinced that I can do a few things with the Listerian dressing that cannot be done without it. There is no way in which you can keep a wound free from odor and check profuse suppuration so quickly as with the antiseptic dressing. I am as sure of that as I am of my own name. I have never seen a major amputation which has not suppurated more or less. Usually it ceases in a short time. I have never amputated a breast under the system in which there was not some suppuration. I have known cases operated upon in our hospital where there was no pus. I have treated them with other dressings, and suppuration has not occurred. But, taking things from first to last, I think that the Lister dressing gives less suppuration than any other method of treating wounds.

In herniotomy, and in abscesses like psoas, lumbar, and those in the iliac region, I think that the Lister method has shown itself more useful than in any other thing. I do not know of any other method by which an abscess will heal more quickly than by the Listerian method. It is only by the working, every-day surgeon that this question can be decided. Some cases will get well anyhow. A man may have an iron bar shot through his head and get well, another have his arm torn off and get well It is not worth while to consider individual cases. The advantage of Listerism in ovariotomy has been fully demonstrated. Spencer Wells has come out fairly and squarely for Listerism in ovariotomy, and if he approves the method, that is enough for me. Dr. John Homans, whose results are noted by the author of the paper, is as correct as any man in this world. I would believe him as quickly as I would the honored President of this Association; and I can say no more than that. Homans, in sixty-five cases, has lost one from hemorrhage, because he tied the pedicle with catgut; in one case I think the disease was malignant, and that leaves two cases in sixty-five.

When Keith has done eighty cases by any other method than Listerism, he can speak against it.

In joint cases, in those where the removal of foreign bodies from the joints is demanded, and in herniotomies, I think that the Lister treatment is the best. Before this plan came in vogue, you know how chary a surgeon was of opening joints. It is now a common—I will not say an every-day—occurrence, and it seems to me that Listerism is entitled to credit for it.

I wish, in conclusion, to say, that you can do a few things with Listerism that cannot be done in any other way. If the advocates of Listerism say that it will cure everything, they place themselves in the attitude of fanatics, and fanatics are not reliable witnesses.

Dr. D. W. YANDELL, of Louisville, Kentucky.

I have listened to the remarks of the preceding gentleman, but I cannot say that there is anything of a convincing character in his statements. The strongest point in his own mind, at least, appears to be based upon the success and reliability of Dr. Homans; success goes for a good deal. The successes for Listerism will not count for much if we can place in juxtaposition to them the successes without Listerism. Another point was, that, if it were not for Listerism we should not now be opening joints. I admit that, but it proves nothing. If it had not been for Otis's extraordinary behavior with the urethra, I do not think that we should now be performing Bigelow's operation; but I condemn Otis's practices.

In ovariotomy my own experience has been very small compared with that of others. My first five cases and my last seven were done without the spray, and all but one got well. Besides these, I did a lot of cases, as many as fifteen, with the spray, and a number of them died. I do not say that they died because of the spray any more than that the others got well because I did not use the spray. The removal of an ovarian tumor without adhesions is a very simple operation. A large proportion of these cases get well under any treatment; but the difficult cases get well only by the care and the skill of the operator. The greatest successes have always been attained in the operator's last cases, while the greatest failures have been in the first cases.

This question of Listerism can only be settled by waiting, and each one working at it, and after a time, we may be able to determine whether or not Listerism has been a real boon.

Dr. E. M. Moore, of Rochester, New York.

I had not intended to take part in this discussion on Listerism, believing that I had not a sufficient number of facts to justify me in talking upon the subject; but the remark of the last speaker in reference to the spray, induces me to say one thing in regard to its use. He says, he did not lose any cases when he did not use the spray, and he lost some when he did use the spray. My belief is that he lost the cases because he used the spray. That is stating it still stronger than he has done.

When this thing was still new, I was about to perform the operation of ovariotomy; but, before I determined to use carbolic acid, it occurred to me to make an experiment, for I thought that the general rush of air might carry with it various things from the air. I then took a steam atomizer, and allowed the spray to fall upon a piece of plate glass for about five minutes. Then I took a lens, and examined the glass, and there was not a space of one-fourth of an inch which was not covered with motes. The steam atomizer is a most ingenious apparatus for spoiling the peritoneum. I at once threw it aside. Although I believe carbolic acid to be very good, I would not use the spray in ovariotomy.

I should like to say one word more entirely separate from this topic. Professor Cabell spoke in reference to hospitals and hospitalism. The best kept hospital will once in a while become infected. This has been the history of all hospitals. Is a perfect hospital possible? It is, I think. What may it be? The only way to disinfect a hospital is to burn it. At the present time, you know, it has become possible to build fire-proof buildings, so that it is now practicable to have a hospital built of iron with marble floors, and walls without lath and plaster. Such a hospital would stand five thousand years, and could be made new every year. Take out the furniture and windows, and put laborers in, and give it a coating of whitewash, and then burn it off. There is a perfect hospital.

Dr. J. W. S. Gouley, of New York, N. Y.

I have, within the last ten years, opened the perfectly healthy knee-joint of a child for the purpose of removing a needle which had entered the joint and was embedded in the cartilage of the inner condyle. I opened the joint freely for three-fourths of an inch, extracted the needle, dressed it as an ordinary wound, applied a simple apparatus, and sent the child away. The wound healed

quickly. There was no suppuration, and no Listeric treatment was adopted. I have treated other wounds of joints without Listerism with equal success.

Dr. J. L. Cabell, of the University of Virginia.

The gentlemen have availed themselves of my paper to ventilate their opinions upon things not in that paper. My friend Dr. Cole imputes to me the infirmity—which he says is common to all the rest of mankind, including himself, and the latter I accept most certainly—of having withheld a reference to certain gentlemen who took part in this discussion in England on his side. I would say in reference to that, that I took two representative gentlemen from each side; Mr. Wells and Mr. Thornton from the antiseptic side, and Mr. Tait and Mr. Keith from the other. I was surely doing no injustice in that. He specifies Mr. Gamgee as having favored that side; but he goes as far as I do in the use of antiseptics in ovariotomy. I think it worthy to inquire whether the use of antiseptics may not be useful in counteracting discharges, and correcting unavoidable defects in sanitary arrangements. I might name besides Gamgee several other surgeons who testified to the value of antiseptics minus the spray. Many of the French and German surgeons spoke in its favor.

TREATMENT OF FRACTURES OF THE SKULL,

Recent and Chronic, with Depression.

By MOSES GUNN, M.D.,
PROFESSOR OF SURGERY IN THE RUSH MEDICAL COLLEGE, CHICAGO, ILLINOIS.

[Read June 1, 1882.]

The subject expressed in the title of this paper was assigned me by the venerable President of this Association at our last annual meeting. The question which it involves is one of treatment only in cases of depressed cranial bones, and turns entirely upon the management of the depressed portion; for there can be no question as to the general treatment of the patient, nor of the wound of the scalp when such wound complicates the case. As to the method of treating the depressed portion, however, there may well be great diversity of opinion, and the object of this paper is simply to introduce the subject for discussion by giving my own views and my reasons for those views.

I have chosen this way of meeting the question rather than the more laborious and oftentimes unsatisfactory one afforded by opening a correspondence with individual surgeons of the country for the purpose of obtaining the results of their personal experience, and it is quite probable that the sentiments which I shall advance may not meet with general approval; perhaps they may encounter general condemnation. But honestly and confidently entertaining them, and for the purpose of opening the discussion, I shall offer them unreservedly.

The general sentiment of the profession, at least since the teachings of Cooper and Abernethy, in England, and Dupuytren, in France, has been decidedly in favor of non-interference by surgical procedures with the depressed portion in cases of simple fracture with depression, except when accompanying symptoms

of compression of the brain seemed to point to the depressed bone as the direct cause of such compression. In compound fractures with depression there has been less uniformity of sentiment; but it has been generally in favor of surgical interference. As fairly correct indices of recent prevailing professional sentiment, I make the following references and quotations:—

Mr. Prescott Hewett, in his article on "Injuries of the Head," in Holmes's System of Surgery, says, "It is now an established rule in our metropolitan hospitals that simple fractures with depression and without symptoms are to be left alone. The depression may be so marked as to be easily detected; and yet so long as there are no symptoms all operative influence of whatsoever kind is to be carefully avoided." Even in depressed fractures *with* primary symptoms of compression, he says, "If the fracture is a simple one and the symptoms are not very urgent, we may postpone the operation." In compound cases of depressed fracture without symptoms he recommends elevation; with symptoms, of course, elevation is imperative.

Mr. Bryant is even more positive and uniform in the practice of non-interference. He says, "In cases of depressed fracture ought the bone to be elevated? and should the fact of the fracture being compound influence the decision? I have no hesitation in answering both questions, and asserting that in neither instance ought surgical interference to be thought of unless the symptoms of compression are marked and persistent."

Mr. Gant uses the identical language of Mr. Hewett on this point, and attributes to Sir Philip Crampton the following statement: "In Dublin we conformed generally to the rule originally laid down by Dease, who preceded Desault by many years, that in fractures of the skull with depressed bone, whether complicated by wound of the scalp or otherwise, no attempt should be made to raise the depressed bone unless very decided symptoms be present of compressed or irritated brain."

Ashhurst says, "Probably few surgeons at the present day would think of operating in a case of simple depressed fracture without symptoms of compression;" and in cases of compound

fracture with depression and accompanying symptoms of compression, he would limit the operation within very narrow bounds.

Hamilton, in speaking of this class of cases, namely, simple fractures with depression and without symptoms of compression, says, "The question is definitely settled in the minds of most surgeons who have had a sufficiently large experience, that it is seldom or never advisable in such cases to operate." If, however, the fracture is compound he says that "prompt surgical interference is invariably demanded."

Erichsen seems more inclined to resort to surgical procedures, and thinks "that the expectant treatment should not be followed too implicitly;" although he, in common with most others, makes the compound character of the fracture the determining circumstance which indicates operative interference. Speaking of this class of cases, he says, "So far as my own experience is concerned, which is necessarily drawn purely from civil practice, I can say that, with the exception of the case that has just been referred to, I do not recollect ever having seen a case recover in which a compound depressed fracture of the skull occurring in the adult had been left without operation; but I have, on the other hand, seen several instances of recovery in which the bone had been elevated and the fragments removed."

Gross goes much further in advocating active surgical measures. In reference to simple depressed fractures, "but not sufficiently far to be productive of compression," he says, "If on the other hand the bone is forced down considerably, so as to impinge very decidedly upon the brain, or if it be comminuted or jagged at the edges, the sooner it is raised or removed the better." With these views of surgical interference in simple depressed fracture without symptoms, his resort to the necessary surgical operation in compound cases is prompt and thorough.

In military practice the expectant plan has generally been the rule; perhaps even more constantly so than among civil surgeons. The exigencies of the service have generally required a neglect of the primary operation in cases without symptoms of compression whether simple or compound; and oftentimes cases with symptoms have necessarily met with the same treatment.

Punctured fractures, on the contrary, have usually been considered as requiring prompt surgical interference whether attended by symptoms or not; and this not so much by reason of the compound character of the accident as from a recognition of the probable comminuted and depressed state of the internal table.

These references are made as indicative of the general sentiment and practice of the English-speaking profession; and, as at the present time the general principles of surgical science prevailing in America and Europe (continental as well as insular) are in tolerable accord, they may be regarded as a fair exponent of professional opinion throughout the civilized world.

And now a question arises so naturally that it seems to spring as a normal growth from the surgical field in its present state of cultivation: Are we not too conservative in the treatment of simple fractures of the skull with depression and without symptoms? I have put the question in the present rather than a past tense because of the modification and improvement of surgical practice in the treatment of wounds consequent upon a recognition of a great truth in so-called Listerism. Without stopping to consider or discuss the truth or falsity of Lister's doctrine of causation in reference to decomposition and suppuration, we cannot fail to recognize the great benefits which are derived from that perfect cleanliness which is realized from thorough drainage, antiseptic applications, and approximative exclusion of atmospheric air from the wound. Joints and the great cavities are now invaded with comparative impunity. Gastric resections of slight ultimate promise are successfully made, and abdominal surgery is bold, aggressive, and victorious, while the peritoneum is as susceptible to a rapidly fatal inflammation as are the cerebral meninges. Compound fractures and resections through cancellar bone structure are shorn of most of their previous danger. May not a surgical interference which extensively lays bare the meninges, or even the brain substance itself, profit equally by our improved methods of treating wounds? The answer of this question, which I assume must be in the affirmative, considered in connection with the peculiarities of fracture

of both skull-tables, gives, also, an affirmative answer to my first question, viz: Are we not too conservative in the treatment of simple fractures of the skull with depression and without symptoms? We certainly are if we continue the conservative methods which we practised previous to the improved modes of managing wounds which now prevail. Non-interference in accordance with most of the authorities quoted *was*, perhaps, correct previous to the late improvement in treating wounds; it is incorrect now. If Dr. Gross, when he said, in reference to his advice to elevate a markedly depressed bone in simple fracture unattended by symptoms, "I am fully, indeed painfully, sensible of the responsibility which I incur in giving this advice," was correct in giving that advice, is it not our duty now to extend the application of his directions to all fractures with depression, however slight, when symptoms of severe concussion or collapse do not contra-indicate all operative procedures? If Dr. Gross was correct in his then advanced position, as I confidently believe he was, we are now fully warranted in making his directions include slight as well as severe cases.

The reasons for such extension of his rule are—

First. In all fractures from an outwardly operating force, it is well known that the injury to the internal table is more extensive than that inflicted upon the external; that the fracture is more comminuted and displaced to a greater extent.

Second. The general principle that a fracture should be reduced applies with extreme force to a fracture where the depressed fragment or fragments impinge upon the great nervous centre, constituting a point of continual irritation. On this point Erichsen says, "The presence of depressed and spiculated fragments pressing into the dura mater must inevitably and speedily induce encephalitis." This irritation of which Erichsen speaks so positively is, occasionally at least, augmented at a later period by the formation of osteophytes at the consolidating lines of union between the fragments, for although as a rule no provisional callus, in the ordinary acceptation of the term, forms on the inner surface of fractured cranial walls, osteophytes do sometimes spring from the new bone formation which unites the

fragments, and by their needle-like projections give rise to future trouble and compel surgical interference at a period more or less remote.

Third. The dangers and complications arising from the compound character of the wound being reduced to nearly if not actually zero by antiseptic procedures, we are warranted in opening up a simple fracture with depression for the purpose of correcting the displacement.

Fourth. As in compound fractures the removal of loose fragments or the elevation of the depressed portion of bone, even though requiring the use of Hey's saw or the trephine in order to effect such elevation, does not materially add to the dangers of the case, so now in a case of simple fracture, which through improved methods we venture to make compound, we may confidently proceed to a full correction of the displacement. Such operative procedures are, however, contraindicated when symptoms of violent concussion with the attendant tendency to collapse are present; but in the absence of symptoms, or when the symptoms point to compression instead of concussion, operative measures should be promptly instituted.

In connection with this I venture to put in a plea for the much-dreaded trephine. Statistics which apparently show against this instrument by exhibiting a large percentage of deaths following its use should also show an absence of inflammation or concussion before its use was resorted to, in order to have any value at all as evidence against it. The operation of removing a disk of bone from the cranial walls, performed with proper care and skill, is not an operation "fraught with danger." There is very little risk of injuring the dura mater with the instrument. If that membrane has not been wounded in the occurrence of the accident for which we resort to the use of the trephine, its safety from injury by that instrument may be pretty confidently assured.

And while I thus defend the proper use of the trephine, I desire also to protest against the use of the chisel or gouge on the cranial walls, if the propelling force is applied by a series of blows from a mallet. A single blow of the force usually required may be insignificant, but the result of a number of them in rapid

succession must be proportionate to the aggregate amount of all the blows. Thus estimated we may not be surprised at symptoms of concussion which sometimes follow its use.

Chronic fractures with depression remain to be considered. By this term I suppose reference is had to those fractures in which whatever primary symptoms may have existed they have now subsided, and a consolidation more or less advanced has taken place between the fragments in their displaced position. This consolidation is occasionally attended also by the formation of osteophytes, which spring from the new bone formation which fills the slight interspaces of the fragments. When this chronic condition of depressed fracture is attended by symptoms of cerebral irritation of sufficient gravity to seriously impair the general health or give rise to fears of a progressive tendency in the morbid condition, even though not yet seriously impairing the general health, the time has arrived for surgical interference. The neglect to resort to a primary operation now calls for redress, and that redress consists in the removal of so much of the cranial walls as will include the whole, or at least the most markedly depressed portion, of the depressed fragment. Although the results of these secondary operations do not show a flattering percentage of success, I think that the reason may be looked for in the late period at which the operation is generally performed. It is rare that the patient submits to the dreaded operation till years have been wasted in the vain endeavor to effect a cure by medication. In the mean time the constant irritation has begotten a permanent impression upon the brain and nervous system which remains after the offending point of irritating bone has been removed. To avoid this unhappy condition the operation should be resorted to on the supervention of positive though slight evidences of cerebral irritation. I venture the opinion that the successful cases of secondary trephining will be found to be those in which the operation has been early resorted to.

In conclusion, I formulate my convictions on the subject thus:—

First. In all recent fractures with depression, whether simple

or compound, even though entirely without symptoms of compression, if there is reason to believe that the internal table is depressed, and if there are no symptoms of marked concussion or collapse, elevation of the depressed portion should be promptly effected.

Second. In chronic cases, as soon as positive even though slight symptoms of cerebral irritation present themselves, a disk of the cranial walls intended to include the irritating point should be removed by the trephine.

Third. All operations and dressings should be conducted upon strict antiseptic principles.

DISCUSSION.

Dr. R. J. Levis, of Philadelphia, Pennsylvania.

I think it should be considered an axiom in surgical procedure that the extent of mechanical depression should more influence the surgeon in regard to the question of operative interference than the symptoms of compression. It should be borne in mind that the symptoms of concussion and shock may mask the symptoms of ordinary compression. I think, from my own experience in the Pennsylvania Hospital (and I believe that my colleagues will agree with me), that the majority of cases admitted with fracture of the skull show more frequently the symptoms of concussion and shock. If then we are to wait for the ordinary symptoms of compression, as the books tell us, I think that we shall very often leave undone operations which should have been performed early. I think that the practice of my colleagues is to elevate in simple fractures where the depression is considerable, being influenced principally by the extent of the depression and the position on the skull where the depression takes place. But the particular point which I wish to impress, is that we cannot be governed by the symptoms of compression, on account of the symptoms of shock and concussion masking them.

Dr. E. M. Moore, of Rochester, New York.

I should like to ask Dr. Gunn a question. How are we to know

in the beginning that the internal plate is so pressing as to do injury, unless we have the symptoms, known and described as symptoms of compression?

Dr. GUNN. I do not know that we can tell anything about that in a case of simple fracture, unless we can assume it upon general principles. In many fractures where the external table is depressed we may assume, I think, except in the regions of certain sinuses, that the internal plate is much more fractured and much more depressed.

Dr. MOORE. That is universally understood, but that is not the point. I have understood you to say that in a case of simple fracture it would be proper to cut down upon it without the symptoms which are called the symptoms of compression being present, provided there is reason to believe that depression of the internal plate exists. How is that known?

Dr. GUNN. In all cases of fracture, simple or compound, with depression, if there is reason to believe that the internal plate is depressed, we should elevate. Now, I repeat, if a fracture with depression exists, and if fracture and depression do not exist in the vicinity of any of the sinuses, we may assume that there is depression of the internal plate.

Dr. MOORE. It is then assumed, because depression of the external plate is present, that the internal plate is also probably depressed, and the statement is: that where there was reason to believe that there was depression you would elevate? I must confess that I was not prepared for this. In cases of compound fracture, I cannot see that there would be any danger in elevating, except the danger from using the trephine. In regard to the danger of puncturing the membranes, I think that must never occur in the hands of a skilful surgeon. The trephine makes a lacerated and contused wound. The vessels of the diploë are torn by the instrument, thus giving rise to more or less danger of septicæmia. I must confess that I have a reluctance to use it, and avoid it when I can. I always first resort to the elevator, pick out the little pieces, use Hey's saw, and get elevation, if possible, without the trephine. One word more. The remark was made by Dr. Gunn, that we might, without injury, puncture the membranes.

Dr. GUNN. I did not advance that opinion. I said that, with the advantage gained by antiseptic surgery, we might encounter with

comparative safety an extensive exposure of the meninges, and even of the brain substance itself.

Dr. Moore. I doubt whether antiseptic surgery would have much influence in such conditions. As to the mode of dressing scalp wounds: These heal more quickly than other wounds, and if sutures are introduced, they will produce suppuration if left too long. My custom always is to resort to twisting the hairs in the place of introducing sutures.

Dr. Hunter McGuire, of Richmond, Virginia.

I think the advice of Dr. Gunn, that we should elevate every depressed fracture, even if it is simple in character, is a very dangerous doctrine. I have lately been looking over some statistics of the mortality following operative interference in fractures of the skull, in the practice of English and American surgeons. The mortality in the hands of the English surgeons was very great when compared with that of the American surgeons. This was apparently due to the fact that American surgeons interfered at once, when interference was demanded, while English surgeons seemed to think it necessary to wait a day or two for symptoms of compression or of inflammation. I think that we are indebted to Dr. S. D. Gross more than to any other man for encouraging surgical interference in compound fracture of the skull. That this is right, almost the whole surgical world now agrees; but to say that it is proper to operate in every simple, depressed fracture of the skull, when no symptoms of compression or irritation are present, would be to give license to every young surgeon to use the trephine, which would be exceedingly dangerous. If the fracture is a simple one, and the depression is not sufficient to bring on symptoms of compression, it is better to wait and see if the brain will not be able to accommodate itself to this condition. We all know that there is no relation between the amount of depression and the symptoms of compression. I have seen a depression of one inch without any symptom of compression. Heretofore we have been governed by the symptoms of compression in determining the necessity of elevation.

Dr. J. W. S. Gouley, of New York, N. Y.

I should like to say a very few words concerning the kind of fracture known as punctured fracture. Dr. Gunn has alluded to this.

Professor Gross may remember my showing him a case of this kind in Washington in 1861. It was the case of a drunken soldier

who had been struck on the head with a musket by a sergeant who was arresting him. The cock of the gun had made a slight depression in the external table. The man was brought to me in a state of insensibility. Within two hours after the injury, I applied the trephine and removed a disk of bone, and with this disk removed all the fragments; under these fragments there was a clot of blood. There was very little depression, yet the symptoms were very clear and marked. I believe, myself, that it was the compression made by the effusion of blood that had caused the symptoms. I believe that the slight hemorrhage saved the patient's life, for had he not been trephined, he would have had grave symptoms following, perhaps weeks or months afterward. I believe that in this class of cases, even if the symptoms are not well marked, it is wise to operate within the first thirty-six hours. I am not advocating interference in every case, but in cases of so-called punctured fracture where there is reason to believe that the internal table is fractured. I do not know how many times I have done the same operation with the same successful result, but I do remember two or three other cases of the same sort.

A second one came to my clinic in 1865. He was a miner from Pennsylvania, a tall, athletic, healthy man. He had been struck on the head by an angular piece of coal. He suffered a great deal of pain, but walked about, and in five or six days made his way to New York, complaining of a sore head. I examined the head, and found a small wound. I advised an exploratory operation. I made a free crucial incision, and found a slight depression of the external plate. I thought that the force continuing had broken the internal plate, but he had no symptoms. Still I trephined this man, and removed seven or eight fragments which had been pressing upon the dura mater, but had not pierced it. There was very little blood in this case. I should have said before, that the bone at the seat of injury was rather white, as though it was dead, or was going to die. Within two weeks after this operation, I noticed that for nearly an inch around the disk the bone becoming perfectly white. At the end of six weeks I must have extracted an inch and a half of bone on one side of the opening made by the trephine. Whether I had killed the bone, or whether the blow had killed it, I cannot say, but I fancy that the original injury had as much to do with it as the trephine.

Within one week of the occurrence of this case I had a similar

case. I operated and got a similar result. Dr. Lidell has advanced the theory that when the skull is struck by a spent ball, no fracture having ensued, there will be necrosis of the bone due to fracture of the elements of bone—the Haversian canals, etc.

Before closing I shall mention another case that I recollect very distinctly. This was a case in which I did not apply the trephine. During the riots in New York in 1863, I had in the hospital about one hundred wounded men, women, and children. I had charge of all the service at that time. There were a number of head injuries produced by brick-bats. Of one of these I have a distinct recollection. It was a policeman who had been struck about two inches above the external angle of the left eye. There was a marked depression; I could put the tip of my finger into it. There was no external wound, no symptoms, and I did not trephine; there is still this distinct depression. That man is now in good general health, though within six months after the injury he was laboring under a mild form of insanity, which is now no worse. This is, I think, one of the chronic cases alluded to by Dr. Gunn in which ossific deposits occur.

Dr. H. F. Campbell, of Augusta, Georgia.

I have ever regarded trephining as one of the most serious of all capital operations. I may have been too much afraid of it, but I believe in the old rule, never to trephine unless there are symptoms of compression. While I commend the care with which Professor Gunn has investigated this subject, and while I am ever friendly to advance, yet I must agree with my friend, Professor McGuire, in saying that it would be a dangerous doctrine to teach that all cases of depressed fracture should be subjected to operation.

I am not willing to admit that because the external plate is depressed the internal plate must necessarily be in the same condition. We may, I think, have depression of the external table without fracture of the internal. There is in some places a space occupied by the diploic structure, which is spongy, and which will, I believe, admit of depression of the external plate without producing pressure on the brain.

As Dr. Gouly has just said, we should always trephine in punctured fractures. I believe that is a part of the old rule which says, "Trephine when you see symptoms indicating compression, but do not trephine when you do not see such symptoms, except in cases

of punctured fractures." In punctured fractures the internal plate is depressed, and although there are no symptoms of pressure at first, there may be severe ones afterward. I have experienced bad results from not trephining in a case of this character. I recall the case in which a man was stabbed in the forehead with a knife. He had no symptoms of pressure whatever. I was without much experience as a surgeon at that time, and in consequence of the man having no symptoms of compression I did not trephine. I could pass a delicate probe for some distance. I thought that possibly there was no splintering of bone. He had convulsions, but recovered, but was never in perfect health afterward. Whether the puncture had caused splintering of the internal plate, or whether the blade had passed through the dura mater, causing injury to the brain, I cannot say. I believe in his case that the trephine should have been applied. On one occasion I was called to see a man with an axe wound of the head, where the brain was exposed, and I found him eating his breakfast. I could not press the pieces of parietal bone back into place, and I had to remove a portion of the edge with Hey's saw, in order to bring them in apposition. The wound united by granulations, and the man recovered without any subsequent bad results.

Dr. R. A. Kinloch, of Charleston, South Carolina.

I propose, sir, to make a very few remarks, and confine myself to simple fractures with depression, and the question of interference or non-interference. I cannot forget the experiences of the older surgeons. I cannot but remember the statistics in regard to many fractures that are not interfered with. I disagree with the broad principles laid down by the author of the paper. If there be moderate depression, without symptoms of compression, and which we cannot positively say involves both plates, I am not in favor of elevating. An exploratory incision, I think, adds to the risk. I think the operation, if justifiable, is only justifiable upon the presumption that the patient is going to suffer from secondary complications, which presumption I contend is not clearly demonstrated by statistics. Taking the case after the occurrence of the injury, a moderate depression is elevated, but the symptoms of pressure continue. The patient dies, and the autopsy reveals that the symptoms have been due to deeper and more serious injury, and that the depression was not the cause of the symptoms presented.

In regard to the risk, I think that unless we adopt the dogma of Mr. Lister, that there is no harm in making any wound under the antiseptic system, we must admit that a mere incision for the purpose of diagnosis increases the risk. I think that the admission of air must necessarily increase the risk. It certainly does not detract from it. As long as the outer table is pressed against the inner, there is the minimum amount of risk; but if the part is opened and the bone is raised with the elevator, more injury is inflicted than has been done by the original injury, and the risks are increased. Autopsies have often shown that fractures involving the inner table have been rounded off, and there is scarcely a remnant of the injury left. In conclusion, I must say that I am a warm advocate of conservative surgery in these particular cases.

Dr. S. W. Gross, of Philadelphia, Pennsylvania.

I have a few remarks to make on the propositions laid down by Professor Gunn. Of course we cannot lay down any hard, positive rules in regard to fractures of the skull with depression.

If I had formulated the propositions, I should have done so in this manner. I would accept unreservedly the second proposition. I do not think that that proposition requires any discussion.

In regard to the first proposition, I would modify it to read: "In all recent fractures with depression, if the latter be moderate, whether simple or compound, the patient should be left alone." If, however, fixed and severe pain at the point of injury, febrile excitement, increase of local temperature, and a commencing puffiness of the scalp supervene, within a few days after the accident—signs which are indicative of depression of the internal table and the development of pachymeningitis—elevation of the depression should be promptly effected.

In all recent fractures, whether simple or compound, attended with symptoms of compression, the trephine should be resorted to; and the same rule should apply, whether symptoms be present or not, if the depression be considerable and funnel-shaped.

Punctured fractures should invariably be subjected to operation.

Dr. D. W. Yandell, of Louisville, Kentucky.

I concur in what the preceding gentleman has said, and if he will add to his third proposition "and the symptoms of compres-

sion persisting," I shall fully agree with him. We all know how fleeting these symptoms sometimes are. An injury to a child's head is much less significant than an injury to an adult's head. A child's brain more readily bears compression.

In a case of compound fracture, all fragments that are loose or making compression should be removed. I understand the object of the author of this valuable paper is to extend the domain of operative interference. I do not go to the extent that he does of operating on all cases, because I have seen many get well without operation; nor so far as another gentleman, who advocates the operation, because he has seen so many evils follow in the train of untreated fracture of the skull without symptoms.

I think the old rule is not a bad one: That we should not elevate the depressed bone unless the symptoms of compression are persistent.

Dr. Moses Gunn. I have nothing to add to what I have said. I stated in the beginning that the propositions which I should submit might not meet with general approval; indeed, I anticipated that they might meet with general condemnation. The discussion has taken a very wide range. I want, however, to call attention to one or two points. In the first place, I made no departure from the rule in the treatment of punctured fractures; nor have I in the treatment of compound fractures; but in the treatment of simple fractures with depression and without symptoms, I have gone a step in advance of the rules laid down by our distinguished presiding officer.

THE TREATMENT OF TRANSVERSE FRACTURE OF THE PATELLA WITH THE OBJECT OF PRODUCING BONY UNION.

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[Read June 1, 1882.]

An object in all methods of treatment of transverse fracture of the patella is, as in other fractures of bones, the retention of the fragments in apposition. To obtain the conditions requisite for bony union is evidently the intention of all appliances, and its desirableness is generally admitted. A few modern authorities assert that a very short ligamentous union is almost, or quite as advantageous, as a true bony union. I have found but one authority who considers a ligamentous union, with all its impairment and disability, as actually preferable to direct union of the broken surfaces.

The varied appliances for the treatment of transverse fracture of the patella, except one, act by pressure exerted on the fragments through the medium of the overlying integuments. The exception is the apparatus which acts, after penetrating the integumentary and fascial or aponeurotic coverings, directly on the separated portions of the broken bone.

The action of all the appliances which exert their force mediately or indirectly through the intervention of the coverings of the bone is mechanically defective, and, to some extent, is liable to render impossible the proper contact of the fractured surfaces. In the first place, the pressure, in a downward direction on the upper fragment and upward on the lower, implies the necessity of a lodgment of the fitted portions of the apparatus in depressions above and below the fragments. Such hollows or grooves

do not anatomically exist, and can be factitiously created only by improper pressure of the instrument. The tendon of the quadriceps muscle is not inserted on the posterior part of the patella, but is attached to its upper edge and anterior surface over which it is spread. The same, relatively, may be said of the ligamentous attachment of the lower fragment. The force necessary for the fixation of points of pressure of the apparatus, in grooves or hollows above and below the fragments, has a peculiar and most undesirable action in tilting the fractured edges forward, thus causing actual separation. Observation of old cases of the fracture and of many dried museum specimens shows this tilting outwards of the broken surfaces.

This separation anteriorly, even though contact should actually remain at the posterior edges of the fragments, is fatal to the process of bony union. It will also be noticed that the depression of the tendinous insertion into a groove has the effect of increasing muscular tension, and that similar pressure on the ligament of the patella tends to draw the lower fragment away from the upper.

I believe that osseous development as a continuity from the periosteum is one of the essentials of bony union of the fragments. It is known that the articular cartilage of the posterior surface has no action in producing bony union, and a fissured appearance of the layer of cartilage remains even when the fragments have been united by bone.

In Gulliver's experiments on animals, as stated by Malgaigne, when the patella was fractured without injury to its periosteal covering, bony union took place; when, on the contrary, this covering was divided the union was fibrous. I believe with Malgaigne that the cause of the rarity of ossific union is, as he expresses it, that the plastic material thrown out partly by the fractured surfaces, but mainly by the periosteum, cannot fill up the interval between the fragments. A number of museum specimens of ununited fracture of the patella show processes of bone from the periosteum stretching in vain endeavor to bridge over the gap. The continued solution of the plasma in the wide interspace which is flooded with synovial fluid from the joint, and

frequently, also, from the rupture of the bursa in front, is an important element in the failure to produce bony union. To the re-establishment of the continuity of the periosteal covering of the patella must we, in my opinion, look for the most efficient factor in the production of bony union. The fragments of a fractured patella will unite by bony fusion as readily as will other fractures of bones of the extremities, if the essential requirements of any bony union can be maintained.

Another mechanical defect in the action of the usual forms of apparatus for the treatment of this fracture is based on the fact that the patella is not a symmetrical bone, the upper edge not being a horizontal line, and the effect of the pressure exerted by the usual treatment is to tend to produce control only at the outer edge of the line of fracture. The most outer part of the upper edge is about one-third of an inch higher than the inner edge. Thus a lateral rotation in an inward direction is produced. Clinical observation of cases and of dried pathological specimens shows the prevalence of this deformity as a permanent result. Hamilton remarks the tendency of the inner portion of the ligamentous union to be the longest, and this deformity is undoubtedly due to greater downward pressure on the outer edge of the upper surface. The same observation is made by Boyer, and Malgaigne states, that "in fractures united by short fibrous bands the outer portion is always the shorter.

The effect, then, of the usual appliances is to prevent coaptation by pressure on the tendon of the quadriceps and on the ligament of the patella, deviating their lines and increasing tension, and tilting the fractured surfaces anteriorly. To this mechanical disadvantage must be added unequal action on the oblique superior edge of the upper fragment, tending to force downward its outer edge and tilting the inner edge upward, thus preventing contact of the torn periosteal edges.

The ordinary apparatus is also objectionable on account of its making pressure on the inflamed structures of the joint and its surroundings. Blood and synovial fluid are always, to a greater or less extent, effused into the joint cavity between the fragments, and frequently into the bursal sac in front of the patella, which

appears to be always ruptured when much separation exists. The unequal pressure of the apparatus increases the inflammatory condition. The constricting action also of the contrivances, many of which act in an encircling manner around the fragments, impedes circulation, and must be a factor in preventing bony union.

The failure to produce bony union means, for the patient, lameness to an extent proportionate to the width of separation. I have not very frequently had the opportunity of following cases of ligamentous union long after the discontinuance of treatment, but some have had to abandon their accustomed occupations of labor and activity. Authorities on the subject, from the time of Paulus Ægineta, have noted the impairment of function of the knee following transverse fractures of the patella. Ambroise Paré declared that he never saw a single patient cured so as not to limp. Fabricius Hildanus made the same statement. Velpeau says that he has never seen the function of the joint completely re-established with an interval of two or three inches between the fragments. Malgaigne "has never seen the functions of the joint restored, even when the separation was limited to one-third of an inch." He has observed that "porters lose more than one-half of their capability of carrying burdens." Mr. Benjamin Bell makes "one-third of an inch the limit of separation, allowing of the restoration of the functions of the knee." A great number of authorities, ancient and modern, agree substantially in similar views.

It may, I believe, be asserted that in no case of ligamentous union, which implies, of course, some greater or less degree of separation, can there be a perfect restoration of the full strength and of all the functions of the limb. The patient may, in cases where there is but slight separation, be able to walk at a considerable pace without perceptible lameness, but in any unusual or extreme motion of the joint, as in making a high step in ascending a stairway, or in running, in mounting to the saddle, or in carrying a burden on the shoulders, there is sure to be evidences of lameness, awkwardness, and of a general mistrust of the strength of the limb. The ability to walk long distances

or to run rapidly, is almost invariably lost, the patient becomes sooner fatigued, and there is a want of control and an uncertainty in the use of the limb. There is, necessarily, a mechanical loss of power in the extension of the limb, due to shortening of the muscular fibres, with drawing up from the point of attachment, and also a want of solidity and unyielding nature of their insertion. The upward displacement of the patella must also render abnormal its action as a pulley and as a fulcrum for the leverage action of the femur and tibia.

There is, as an additional cause of lameness, a want of flexibility, due to inflammatory sequences which are increased by the inordinate pressure of the usual apparatus. This stiffening interferes with flexion, and the lengthened muscular insertion with extension. Sometimes complete extension by muscular action is permanently impossible, and the weight of the body, in the erect position, cannot be transmitted on through the bones of the thigh and leg without a continued muscular strain. This condition does occasionally exist without even great separation of the fragments. It has been noticed that, in old cases with much separation of the fragments, the shortened muscles have undergone atrophic change, were shrunken in thickness, and dissection has shown fatty degeneration.

The disability due to permanent separation of the fragments has also the fault of obliging patients to put an excessive amount of labor, and to throw the weight of the body, on the sound limb. From this cause, during extraordinary efforts, the patella of the sound side is frequently broken.

The important question is raised as to the comparative merits of bony and ligamentous union of fracture of the patella. Bony union is, with the ordinary treatment, so rare that the question has certainly not been settled by practical observation of many cases. Of one hundred and twenty-seven cases of fracture of the patella, recorded by Hamilton, none were known to have united by bone. In a conversation with that distinguished authority, he expressed to me his doubt as to complete synostosis of the fragments ever being produced by any means of treatment. From analogy of fracture of other bones and traumatism in

general, it is impossible to infer that a result in deformity can be equal to a perfect restoration to the normal condition. As to the greater liability to refracture of true bony union there is the same deficiency of the practical test of experience. My own view of the matter is that bony union—reinforced, as in fracture of other bones, by some development of provisional callus and the re-establishment of the aponeurotic coverings of the bone—is a permanently stronger and more effective union than a yielding ligamentous union. I, at the same time, admit that a very imperfect union with a long interval of ligament and attended with great disability of the limb is, owing to the reduced muscular power, less liable to refracture than a true bony union after a short period of formation. The more defective the union the less will be its liability to refracture, and, where the fragments are very widely separated, muscular action is necessarily so weakened that rupture of the long bond of union is rendered almost impossible. As to the weakness and liability to refracture of deformed union, no argument is needed beyond the fact that of Hamilton's one hundred and eight cases of transverse fracture of the patella, treated by the ordinary appliances, twenty-seven are known to have become refractured; just one-fourth of the entire number. If, from bony union, better results are not to be hoped for, then must the treatment of fracture of the patella remain truly the great and melancholy opprobrium of practical surgery.

In the treatment of transverse fracture of the patella too little consideration has been given to the tendinous and aponeurotic structures which are involved in the injury. It is probable that there is as much strength in these structures to resist fracture as in the patella itself. The continuity of the quadriceps muscle to its insertion might well be described as a tendon with a sesamoid bone in it. This sesamoid bone, the patella, belongs rather to the tendinous system than to the skeleton. The action of the patella is rather that of a pulley to change the direction of force than merely to give strength to the insertion.

DISCUSSION.

Dr. T. G. Morton, of Philadelphia, Pennsylvania.

There are several points on which I do not entirely agree with Dr. Levis. From 1864 to 1873 I used, as had always been the custom in the Pennsylvania Hospital, the old form of apparatus for treating fractures of the patella. I had a great many unsatisfactory results, and it occurred to me to try the original Malgaigne's hooks. In 1873 I had a case of transverse fracture of the patella, in which there was a separation of two inches. I etherized the patient and applied the hooks. The approximation was perfect. This case is described in the "Surgery of the Pennsylvania Hospital." The patient was presented for examination to a number of surgeons, who were unable to tell which patella had been injured, except by the scars in the skin left by the hooks. The fracture seemed almost to have united by first intention. Following this, I had a number of cases in which I used the hooks, in several of which there was considerable joint effusion, and there was a good deal of difficulty in approximating the fragments. I was not entirely satisfied with Malgaigne's hooks. I do not think Dr. Levis was either, for he divided the instrument into two parts. I altered the instrument so that the space between the hooks could be varied, and then screwed together. I have used this instrument satisfactorily in a large number of cases.

In regard to the time of application. I never put them on under four days. I etherize the patient, as this relaxes the quadriceps muscle and makes the approximation easier. I confine the limb to a posterior splint for a few days. I have never seen the hooks drag. In one case where there was effusion into the knee-joint, after two tappings, I applied the hooks, but the joint became swollen with effusion, not in consequence of the hooks, but on account of the previous irritation, and I had to remove them.

I have never seen much use in keeping the hooks applied over sixteen or seventeen days. In ten days consolidation usually occurs in successful cases.

In regard to the introduction of the hooks, I have never found any difficulty in striking the bone, nor have I seen any bad results following their use.

There are many cases of fracture of the patella in which it would not be justifiable to use the hooks; as in cases of comminuted fracture, and where there is continued swelling, and where approximation can readily be made and kept up.

I have here a patient who had a comminuted fracture of the patella, who has had wonderful union, almost bony. It was treated by the ordinary apparatus, showing what good results may be obtained by that method. Transverse fractures are the ones most appropriate for this treatment.

This man (exhibiting the patient) had a badly comminuted fracture of the patella, which occurred from the kick of a horse in 1866. He does not experience the least difference between the two limbs.

In some cases of fracture occurring in obese patients, the hooks are not long enough to reach the bone. For such cases I have made the hooks longer and straighter.

Dr. William A. Byrd, of Quincy, Illinois.

I think that results equalling those described can be obtained by much milder methods. I have seen very few fractures of the patella. The last case I treated with Martin's rubber bandage. I was called to a patient expecting to find a fracture of the leg, but found a fractured patella. I placed the limb upon a pine board a little wider than the limb, and elevated the foot; I made a little projection opposite the joint, and applied an ordinary bandage with figure-of-8 turns. When the effusion became large I tapped and re-applied the bandage. I afterwards took off the roller and applied Martin's rubber bandage, commencing just below the knee, and making figure-of-8 turns about the joint; this pressure would of course tilt the fragment, but by bringing the bandage under the splint and up over the outside of the knee, I brought the parts in apposition. I went to see him on the next day, thinking that I should have to take away the fluid, but I found the fluid all gone, and the parts in apposition. I washed the part, cleaned the bandage, and re-applied it. I continued this treatment for twelve weeks, and I presented the case to the State Medical Society week before last. If the bandage is applied as I have indicated, padding the splint well with cotton, so that the muscles shall not be irritated, and keeping the limb elevated, these cases can be treated safely, pleasantly, and surely. I think that this plan is at least

worthy of consideration. I do not consider that this one case proves anything, but if it can be done once, it can be done again.

Whenever I have a fracture of any bone, and the limb is swollen and I want to gain time, I wrap the limb in the bandage and leave it for three or four days, in order that the continuous pressure may cause absorption. Following this plan, I have had good results, and I am satisfied with it.

Dr. R. B. Bontecou, of Troy, New York.

I believe that ossific union has been obtained by other means than Malgaigne's hooks. I have had a pair of them for a number of years, but have never used them. In my last case, I fancy that I obtained osseous union by the use of extension by adhesive strips. The limb was placed on an inclined plane, and a weight was attached to a cord passing over the pulley at the foot of the bed. I also used circular strips around the front of the knee-joint to keep the fragments from tilting upwards. After recovery, I could not discover any movement in the fragments.

Dr. H. F. Campbell, of Augusta, Georgia.

I would remark that in two or three cases that I have had, I have never succeeded in getting bony union. One of the patients was able to walk without any limping. Another patient passed from under my care, but recovered with some lameness. There was, I think, in this case more than two fragments.

As to the difference between bony and ligamentous union, my observation and experience have been that there cannot be a great deal of difference. I have had two patients in whom I had a long ligamentous union, but they had good use of the limb. The patella is a sesamoid bone which serves to increase the leverage of the quadriceps muscle, and its fracture does not seem to be of very much mechanical importance. In 1853 I presented to the American Medical Association a plaster cast of a case, in which there was congenital absence of both patellæ. The abnormal condition occurred in a negro boy, 20 years of age; there was a flatness in front of the joint, but there was no mechanical difficulty whatever in the use of the limbs. He could dance, run, jump, and make every movement possible, without any trouble.

There may be some weakening of the limb itself from rupture of the patella. It seems to me that while bony union is very desirable,

it is rarely attainable. I have never used Malgaigne's hooks. My method in the last case was to keep the limb extended and use Ahl's splints, and I found that even after the patient was able to walk they afforded support in prolonging the treatment. This patient had a very useful limb.

Dr. R. J. Levis, of Philadelphia, Pennsylvania.

There was a point in the anatomy of the knee-joint to which I called attention, and which I want to reiterate. There are no grooves above and below the patella. They can only be produced by making pressure above and below the patella. This abnormal pressure on the muscle draws the upper fragment upward, and at the same time the pressure on the ligamentum patellæ draws the lower fragment downward, and at the same time the fragments are tilted anteriorly. I have here a dissection made by my associate, Dr. John B. Roberts. This dissection shows what I intended to demonstrate, *i. e.*, why the patella breaks at its lower third, because it is covered by the tendon of the rectus muscle to that point, and the ligament of the patella also terminates there; there is also a leverage over the lower portion of the femur. It also demonstrates the manner in which the tendons of the vasti muscles spread downward and laterally over the joint, and running down to the dense structures of the leg.

I believe the continuity of the bone is always due to the re-establishment of the continuity of the periosteum.

I have been questioned as to the danger in the use of the hooks. I think it will have been seen from the diagram that it is impossible to penetrate the joint; the only difficulty is in imbedding the hooks into the bone so that there can be no dragging.

As to the time of application, I differ from my colleague, Dr. Morton. I do not apply them until the effusion has been absorbed. This generally takes from ten to fifteen days. I think the hooks should rarely be applied before the tenth day. I do not know that this is absolutely essential, but I know that it is easier to draw the fragments into position at that time, than a day or two after the injury.

As to the use of anæsthetics. The application causes very little pain. I use the anæsthetic to relax the quadriceps muscle. The integument must be drawn very tense so as to prevent its being bruised. In all cases in which there is much separation there is

rupture of the bursa. It is a mechanical impossibility to have much separation of the fragment without such rupture. It has seemed to me that in a few cases the rupture has not occurred, for I have found a puffy mass in front of the patella. In such cases it is difficult to apply Malgaigne's hooks.

As to the dressing. I am not in the habit of using any dressing. If there is much tendency to displacement, it is well to elevate the body, so as to relax the rectus muscle. Around and between the hooks, I pack lint or charpie to keep them in position, and apply carbolized oil. I have never had any trouble. All of my cases have resulted either in bony union or what is certainly its equivalent. Some doubts have been expressed in regard to the cases presented. These cases were not selected; they were only looked for this morning. Doubt has been expressed whether they are really cases of bony union. I say, that if it is not bony union, it is as good as bony union. No other method of treatment will secure such results. It is often difficult to say whether or not there is a little movement, but these men have been following active occupations for several years, and if the union was ligamentous the tissue would have stretched.

In reply to Dr. Moore, Dr. Levis said that he never had had an opportunity of making a post-mortem examination in any of the cases which had been treated by Malgaigne's hooks.

SECTION OF THE FEMUR, FOR RELIEF OF BONY ANCHYLOSIS OF THE HIP-JOINT IN BAD POSITION,

With Presentation of the Specimen.

By E. M. MOORE, M.D.,
of Rochester, New York.

[Read June 1, 1882.]

I desire to present to the Association a specimen of such an extraordinary character that I believe it will compensate for the time and attention which may be bestowed upon it. The specimen is one that has been prepared of the pelvis and two thigh-bones of the subject whose case I will now relate.

November 12, 1875, Peter McGue, a convict in prison at Auburn, New York, while at labor on the top of the front wall of the prison yard, was struck by a derrick, and fell to the ground, causing a dislocation of the femur upon the dorsum of the ilium. It was reduced by the method of manipulation. This remained in good condition for about three weeks, until one night he fell out of bed and dislocated the joint again, but at this time the efforts at reduction failed. On the 25th of December, several physicians of Auburn endeavored, in concert, by the employment of the method of manipulation, and also with pulleys, to reduce the dislocation, but were obliged to abandon it; leaving the limb, with the toes extraordinarily everted, the limb shortened two inches, the head of the femur presenting forwards instead of backwards, and lying just behind and below the anterior superior spinous process. It is obvious that this describes an irregular luxation, one not coming under any of the descriptions known as the four regular ones. It is one that I had myself seen, and, indeed, had temporarily produced, in some efforts with great force, in practising the plan of manipulation. The effect upon the

limb is to render it peculiarly useless. The shortening of two inches alone would be a very serious obstruction in walking, but when the motion in the knee-joint was precisely at right angles to the normal position, it is obvious that even if the body should fall down upon this shortened limb no progression could be effected in consequence of this rectangular position.

Thus far the description of the case is derived from the report of Dr. Button, having then control of the hospital in the prison.

On December 7, 1877, nearly two years after the accident, I attempted an operation for its relief. In consequence of a somewhat large and unpleasant experience during the early years that succeeded the revival of the method of manipulation by Dr. W. W. Reed, I became thoroughly convinced that the most advantageous position for the head of the thigh-bone in dislocations is that known as the one in the ischiatic notch, or, as designated by Prof. Bigelow, under the tendon of the obturator. I had also observed in unreduced luxations in this position, which we all know to be rare, that the lameness and pain were really very little. Therefore, if it were possible by manipulation to change the head of the bone from its strange position down into the ischiatic notch, we would so far change the attitude of the limb as to bring the knee and ankle almost in the normal line. Besides, the shortening would be converted from two inches to less than an inch. At the end of two years I would not of course make the puerile attempt to bring the head of the bone into the acetabulum. Although the adhesion to the dorsum of the ilium was firm, I hoped that short ligamentous bands constituted the means of its retention. Accordingly, in order to have the advantage of subcutaneous surgery, I had a knife made as a tenotome, with a rounded shank four inches in length. I hoped that by the aid of this instrument, bands, no matter how short and broad, could be divided when the thigh was subjected to tension by the pulleys. Accordingly, they were applied and faithfully used, with the patient under the influence of ether. Some of these bands were found and divided; but, after a great many punctures were made at every point of the circle surrounding the head, it was ascertained that the anchylosis was of a bony character. Indeed, a huge osteophyte inclosed the head and joined it to the dorsum of the ilium.

Failing thus entirely in the original intention, my mind was directed to the next step in the range of reasonable surgical possi-

bilities. The great desideratum was clearly to turn the knee, ankle, and foot as near as possible in the normal line, and at the same time to gain such additional length of the limb as might be possible. I now made an incision below the trochanter, on a line with the acetabulum, barely large enough to permit the introduction of the finger, down to the thigh-bone, which was notched by a metacarpal saw through about one-fourth of its thickness, both upon the anterior and posterior surface. This enabled me to fracture the femur by using it as a lever. The limb was immediately rotated a quarter of a circle, and the weight attached to it by means of adhesive plaster. The effect of this was to bring the leg down to within half an inch of its normal length. It was maintained in this position for just one month. The first effect of the operation was to produce surgical fever, but of no very serious moment, and I, therefore, omit the details of the case as noted day by day, as unnecessary for this audience. On January 5, 1878, just thirty days after the operation, the patient's condition was good, discharge moderate and of healthy pus. The weights were now discontinued, and the limb was placed on a double-inclined plane. This, however, produced pain and restlessness at night, and increased the discharge of pus. During the whole time a drainage-tube had been kept in the opening. But other openings formed.

The treatment consisted of quinine, stimulants, good diet, and the local application of poultices. As the result of this plan of treatment improvement continued; the angle of the splint was daily changed, in order to create a false joint that might be useful in the future.

At the end of about six weeks, on February 3d, an apparatus such as is used in infantile paralysis was applied for the purpose of keeping the motion in walking in the proper line. With the aid of crutches he continued to improve, walking moderately, and bearing slight pressure upon the sole of the foot. This he could do with slight pain. Pus continued to discharge through the openings.

The apparatus was now discontinued, and he was able in a short time to walk with the aid of a cane. His health, however, was poor; he was subject to occasional attacks of diarrhœa, and at one time a syphilitic eruption made its appearance while he was very much prostrated. This gave a clue to some symptoms which had been ill-defined. The use of iodide of potassium with some mercury

caused a rapid improvement in his condition, and the fistulæ finally closed. But about three months before his death he gave evidence of pulmonary disease, which had progressed so far as to produce several large cavities.

I lost sight of him until July 22d. His term of sentence had now expired, and he was brought to St. Mary's Hospital, in Rochester, where he remained until July 30, 1880, just two years afterwards, when he died with pulmonary phthisis. When he arrived I was surprised to find that, although the limb was shortened scarcely more than half an inch, thus showing that the bones at the time of the operation had been separated an inch and a half, the whole space had been filled in by osseous deposit. My false-joint had utterly failed. I now exhibit to you the specimen. The demonstration of all these facts is capable of being made at the present time.

The success of the operation was very gratifying, notwithstanding the failure of the false joint. If good health could have been attained, the patient would have been perfectly able to have followed his vocation as well as any one with anchylosis of the hip-joint. But, perhaps, the scientific questions are as interesting in this connection as the practical ones. How is it that bone, such as we see here, should have been created so as to fill this huge gap? We all know that if the periosteum could have been saved intact, and a piece of the thigh-bone an inch and a half in length had been abstracted, that although new bone might have been developed from this natural matrix, still this would have been a very improbable result. But here, when the bone was cut with the saw, broken, and afterwards pulled apart, the periosteum must also have been separated to a distance almost if not actually as far as the bone itself. We also know that compound fractures, even when the bones are in contact, furnish us the larger proportion of cases of non-union and false joint. The growth could have been due, as it seems to me, only to a rapid and enormous production of granulation tissue, which also in its turn furnished an easy opportunity for the deposit of bone salts. It is truly remarkable that the form and size of the bone in the intermediate space should have been so well preserved.

DISCUSSION.

Dr. H. F. Campbell, of Augusta, Georgia.

I desire to express my pleasure at having had the opportunity of seeing such a rare case in which the production of bone has been so great. There is one point to which I would like to call attention. In his remarks, Dr. Moore said that he had treated this patient with iodide of potassium and some mercury for a syphilitic eruption, which appeared during the treatment of the case. Could this great proliferation of bone have been due to the syphilitic infection? Could such a condition of bony deposit occur in a perfectly sound and non-syphilitic patient, if placed under exactly the same circumstances?

There is another condition presented in the specimen which I think is quite remarkable, and I would like to inquire what was the condition of the acetabulum in the fresh preparation? I notice that in the prepared specimen it was remarkably clear and empty. Was it filled up in the recent state?

Dr. J. Ewing Mears, of Philadelphia, Pennsylvania.

One point presented by Dr. Moore is in regard to the formation of a false joint after osteotomy. I beg leave to refer to a case which came under my observation some five or six years ago, in which I performed subcutaneous osteotomy for the relief of pain and impaired motion in an old dislocation of the shoulder-joint. The patient had suffered excruciating pain for several years, due to pressure upon the nerves in relation with the joint. I decided to perform subcutaneous osteotomy, and form if possible a false joint. I performed the operation by passing a tenotome through the soft tissues to the bone, dividing the periosteum, and then dividing the bone by Mr. Adams's saw. The patient was entirely relieved from pain. He could make movements which could not be accomplished before the operation. My belief was that I had made a permanent false joint. A year subsequent to the operation the patient died from an overdose of morphia, and I obtained the specimen after a post-mortem examination. I found that there was no false joint. There had been osseous union between the fragments. I had accomplished what I desired; which was to remove the pressure from

the nerves and to afford increased motion; but I had not produced a false joint This question is interesting in connection with subcutaneous osteotomy in cases of bony anchylosis of the hip-joint. I think it doubtful if permanent false joints are secured by subcutaneous osteotomy.

Dr. Moore. I desire to make one remark with reference to the formation of a false joint. I performed the operation for the formation of a false joint on one occasion in the case of a child. It was not in my own practice, and the attending physician did not do as I directed. He had an idea that exercise on a tricycle would assist in forming the joint, and in a short time he put the little fellow on one. This exercise was kept up, and the fragments united by bone, producing the same deformity as was present before the operation.

A word about syphilis in the case reported. I do not think that it is worth while to look so far for an explanation of this extraordinary development of bone. All who have examined the hip-joint pathologically have been struck with the power it has of producing bone. This seems to be determined by some general law. In this particular case the osteophytes were abundant, and they existed before I operated. I do not think that syphilis had anything to do with this development.

In reply to the question asked by Dr. Campbell with regard to the condition of the acetabulum, I desire to say that it was found, at the time of making the autopsy, filled up.

FOREIGN BODIES IN THE AIR-PASSAGES.

A Study of 1000 Cases to determine the Propriety of Bronchotomy in such Accidents.

By J. R. WEIST, M.D.,
of Richmond, Indiana.

[Read June 1, 1882.]

One of the evidences of the advancement of surgical science is that its *dicta* no longer emanate from an individual, but are formulated from the united experience of the profession. Every one, therefore, who adds a new fact, or assists in classifying those furnished by others, aids in their construction, and becomes in part responsible for them. This divided responsibility, as well as the respectful hearing which labor alone secures for even the most obscure student, gives me the courage to appear before you and question whether one of the accepted rules in surgical practice is correct.

Although Frédéric Manavius, in 1644, formally advised tracheotomy for the removal of foreign bodies in the air-passages,[1] and Verduc and Heister called attention in 1739 to new facts, and advised the operation,[2] and Louis, in his celebrated memoir in 1759,[2] after presenting the facts previously published, declared himself strongly in favor of bronchotomy in such cases, it was not until a much later period there was a general agreement among surgeons as to the propriety of the operation in cases of this accident. Indeed, it was only since the elaborate discussion on Foreign Bodies in the Air-passages, by Prof. Gross, in 1854, that this was effected.

[1] F. Guyon, Dictionnaire Encyclopédique des Sciences Médicales, Art. Larynx, p. 725.

[2] Ibid., p. 698.

In the work referred to, Prof. Gross says there has been established "the important practical precept to resort to bronchotomy in all cases, the moment it is known that there is a foreign substance in the windpipe."[1]

That this statement is a clear enunciation of an established rule in surgery may be made evident by a brief examination of some of the leading authorities on the subject.

Prof. Gross, in the work already quoted, in his general summary, says: Inasmuch as no confidence can be placed in other means, "it follows as a necessary corollary, that bronchotomy affords the best chance of relief, and that, consequently, it should always be resorted to as early as possible, unless there is some special contra-indication."[2] In the last edition of his surgery, this opinion is re-affirmed in these words: "Having satisfied himself that the foreign body is in the air-passages, the sooner the windpipe is opened the better."[3]

Mr. Erichsen says: "If a patient be seen a few hours, days, or weeks, after a foreign body has been introduced into the air-passages, or indeed at any period after the accident, and inversion have failed, tracheotomy ought to be performed. And this should be done, even though the symptoms be not urgent."[4]

Mr. A. E. Durham says: "When a foreign body is known to be retained in some part or other of the air-passages the promptest treatment is demanded. The surgeon should not trust to the unaided efforts of nature to expel the extraneous substance; as a general rule, the windpipe should be opened with as little delay as possible in every case in which a foreign body is certainly known to be retained in any part of the air-passages."[5]

Mr. T. Holmes says: "When the diagnosis of a foreign body has been made, the surgeon should allow no delay in removing it at once."[6]

[1] A Practical Treatise on Foreign Bodies in the Air-passages, p. 229.
[2] Ibid., p. 458.
[3] Prin. and Prac. of Surgery (ed. 1878), vol. ii. p. 409.
[4] Science and Art of Surgery (Amer. ed., 1869), p. 388.
[5] Holmes's System of Surgery (Amer. ed.), vol. i. p. 710.
[6] Treatise on Surg., its Princ. and Prac., p. 211.

Says Mr. Bryant: "Given the diagnosis of a foreign body in the windpipe, the duty of the surgeon plainly is to endeavor to remove it. There should be no deviation from this rule. The surgeon should never allow himself to be misled by the mildness of the symptoms, nor by the knowledge that in rare instances foreign bodies have remained in the passage for years and were then expelled, as such cases are exceptional. The accident is one that will inevitably destroy life, although it must be doubtful at what time, or in what form danger may appear."[1]

Opinions, similar to the above, might be easily multiplied, but to add to them would be a needless repetition, as they are sufficient to establish the proposition, that it is at present a doctrine in surgery, *that, as a general rule, the certainty of the presence of a foreign body in the air-passages makes bronchotomy necessary.*

Prof. Gross observes, that "No man, however old or however great his opportunities for observation, can possibly have much experience in this branch of surgery."[2] It therefore follows as a necessary consequence that the rule given rests chiefly on the collective experience that has from time to time been assembled in statistical tables; the most complete and authoritative of which are those of Prof. Gross and Mr. Durham. These tables certainly seem to afford a sufficient basis on which to rest the conclusion, that, in cases of foreign bodies in the air-passages, bronchotomy offers a far better chance of recovery than waiting for spontaneous expulsion. A brief examination of the tables in question will make this apparent.

Prof. Gross's tables[3] contain the particulars of 85 cases of the accident not subjected to bronchotomy. Of these 56, or 65.76 per cent., recovered (including 3 cases of expulsion by use of emetics, and 4 by inversion of the body), and 29, or 34.11 per cent., died (including 8 cases of death after spontaneous expulsion). Bronchotomy was practised in 98 cases; 83, or 84.69 per cent., recovered, and 15, or 15.30 per cent., died.

[1] Practice of Surg., p. 562.

[2] Foreign Bodies in the Air-passages, p. 8.

[3] Ibid.

Mr. Durham reports[1] 271 cases without operation; 156 recovered, or 57.5 per cent.; 115 died, or 42.5 per cent. Cases operated on, 283; recovered, 213, or 75.2 per cent.; 70 died, or 24.8 per cent. Among the cases of Mr. Durham, reported as recovering after operation, are 3 cases of direct extraction, and 12 by inversion of the body and succussion; these should be excluded in estimating the chances of recovery with and without operation. Doing this, there remain 268 cases subjected to bronchotomy; 198 recovered, or 73.88 per cent., and 70 died, or 26.11 per cent.

Mr. Durham, in another table,[2] reports 167 cases of tracheotomy for foreign bodies in the air-passages. Of these 130 recovered, or 77.85 per cent.; 37 died, or 22.15 per cent.

Adding the cases of Prof. Gross to those of Mr. Durham, the result is a total of 722 cases; 356 without operation, and 212 recoveries, or 59.55 per cent.; 144 deaths, or 40.49 per cent. With operation, 366; 281 recovered, or 76.77 per cent., and 85 died, or 23.22 per cent; a difference of 17.22 per cent. in favor of operation.

In 1867 I collected and published[3] the particulars in relation to 163 cases of foreign bodies in the air-passages; the results of these cases were so greatly at variance with the reports of a similar kind previously published, that I was led, on the advice of Prof. Gross, to continue the collection of cases. Accordingly, in 1879, I issued a circular to the profession, asking for a report of cases. This circular was extensively distributed throughout the United States and Europe, and met with a liberal response; and the cases reported, together with a small number collected from medical journals, make an aggregate of 1000. A summary of these cases may be found in the accompanying table, the publication of the general tables being omitted.[4] The chief value of my work perhaps arises from the fact that it is only possible for a small number of the cases to have ever been used in a statistical inquiry, as 897 are reported as never having been

[1] Holmes's System of Surgery, vol. i. (Amer. ed.), pp. 709-10.

[2] Ibid., p. 714.

[3] Trans. Indiana State Med. Soc., 1867.

[4] See note at end of paper.

published.[1] A large amount, therefore, of new material is presented for use in determining the validity of the surgical rule previously given in cases of foreign bodies in the air-passages.

As the subjoined table contains in a compact form all of the facts important for statistical purposes, it would be a needless repetition to reproduce other than the leading ones here.

In 63 cases the foreign body was removed by operative measures other than bronchotomy, *e. g.*, with forceps, with or without the aid of the laryngoscope, etc.; these are excluded in calculating the chances of recovery afforded by bronchotomy, compared with those following the plan of non-interference. There remain 937 cases; of these 599 were not subjected to bronchotomy, 460 recovered, or 76.79 per cent.; 139 died, or 23.20 per cent.

Bronchotomy was performed in 338 cases, with 245 recoveries, or 72.48 per cent.; 93 patients died, or 27.42 per cent.; a difference in favor of non-interference of 4.31 per cent.

In cases without operation the table shows 11.03 per cent. more recoveries than do those of Prof. Gross, and 19.29 per cent. than those of Mr. Durham, and 7.24 per cent. than the aggregate of the cases of Prof. Gross and Mr. Durham.

In the cases in which bronchotomy was performed, the table shows 12.21 per cent. less recoveries than those of Prof. Gross, 1.40 per cent. less than those of Mr. Durham, and 4.29 per cent. less than the united cases of these gentlemen.

Combining the cases here reported with those of Prof. Gross and Mr. Durham, the result is a total of 955 cases without operation, of which 283, or 29.78 per cent., died. The sources named furnish a total of 719 bronchotomies, with 178 deaths, or 24.75 per cent.

As a result of this study of 1674 cases, it appears that without operation there is 1 death in 3.5 cases, and 1 in 4 after bronchotomy.

[1] The cases collected from original sources, and reported to me as unpublished, contained in my report to the Ind. State Med. Soc. in 1867, I here report as unpublished, as this is but a continuation of that work.

The cases of laryngotomy reported by Prof. Gross give 76.52 per cent. of recoveries, and 23.50 per cent. of deaths; cases of laryngo-tracheotomy, 76.92 per cent. of recoveries, and 23.8 per cent. of deaths; and the cases of tracheotomy, 88.23 per cent. of recoveries and 11.76 per cent. of deaths.

Mr. Durham reports 76.52 per cent. of recoveries after laryngotomy, and 23.50 per cent. of deaths; 75 per cent. of recoveries after laryngo-tracheotomy, and 25 per cent. of deaths; and 73.59 per cent. of recoveries after tracheotomy, and 26.40 per cent. of deaths.

In the table appended, 36 cases of laryngotomy give 30 recoveries, or 83.33 per cent., and 6 deaths, or 16.66 per cent.; 26 cases of laryngo-tracheotomy give 19 recoveries, or 73.07 per cent., and 7 deaths, or 26.93 per cent.; and 276 cases of tracheotomy give 196 recoveries, or 71.02 per cent., and 80 deaths, or 28.98 per cent.

The tables of Prof. Gross, Mr. Durham, and my own, give 70 cases of laryngotomy, with 56 recoveries, or 80 per cent., and 14 deaths, or 20 per cent.; 59 cases of laryngo-tracheotomy, with 44 recoveries, or 74.57 per cent., and 15 deaths, or 25.42 per cent.; and 605 cases of tracheotomy, with 449 recoveries, or 74.21 per cent., and 156 deaths, or 25.78 per cent.

It appears from this calculation that after laryngotomy for foreign bodies in the air-passages, 1 patient in 5 dies, and after laryngo-tracheotomy and tracheotomy, 1 in 4 dies.

Although this study of the combined tables yields results slightly different from those furnished by my own—being a little more favorable for bronchotomy—the difference is too slight to furnish an argument in support of the existing surgical rule. This becomes apparent when the cases here presented are examined alone.

If the table is examined, the fact will be disclosed that a study of the new material presented does not tend to establish the correctness of the opinion, that the presence simply of a foreign body in the air-passages determines the necessity of bronchotomy.

The foreign body that most frequently finds a lodgment in

the air-passages is a grain of corn (maize). 177 examples are here presented. In these cases, spontaneous expulsion followed by recovery occurred in 66, or 71.74 per cent. There was a fatal result in 26, or 28.26 per cent., cases without operation. Bronchotomy was practised in 85 cases, 66 recovered, or 77.64 per cent., and 19 died, or 22.36 per cent.

In 109 cases, a watermelon-seed (Cucumis citrullus) was the extraneous substance. Of the 75 cases without operation, 70, or 93.33 per cent., recovered, and 5, or 6.66 per cent., died; and of the 34 cases in which bronchotomy was practised, 26, or 76.47 per cent., recovered, and 8, or 23.53 per cent., died—a death-rate 16–16.87 per cent. greater than in the cases without operation.

In 90 cases, the foreign substance was a bean. Of 51 cases without operation, 30, or 58.82 per cent., recovered, and 21, or 41.17 per cent., died. Bronchotomy was performed in 39 cases, 24, or 61.54 per cent., recovered, and 15, or 38.46 per cent., died —a mortality 2.71 per cent. more favorable than in the cases without operation.

In 59 cases, a grain of coffee was lodged in the air-passages. Of the 34 cases without operation, 29, or 85.29 per cent., recovered, and 5, or 14.71 per cent., died—a death-rate 29.29 per cent. greater than in the cases without bronchotomy.

Under the head of "seeds of various kinds" are found 94 cases; 58 without operation, and 47, or 81.03 per cent., recoveries, and 11, or 18.96 per cent., deaths. In 34 cases operated on there were 27, or 75 per cent., recoveries, and 9, or 25 per cent., deaths, or 7.04 per cent. more than in the cases without operation.

In 371 cases, in which the foreign body comes under the head "miscellaneous," 263 cases were without operation. Of these 199, or 75.67 per cent., recovered. In 108 cases of bronchotomy, there were 77, or 71.29 per cent., recoveries, and 31, or 28.71 per cent., deaths; a number 4.38 per cent. greater than in the cases without operation.

In presenting these facts, I am not seeking to bring bronchotomy into discredit in cases of foreign bodies in the air-passages. In a large number of cases the larynx or the trachea must be

opened to save life, and the surgeon who fails to urge the necessity of prompt operation will be neglectful of his duty. I am only striving to show that the present accepted rule is too broad, that in many cases where it is certainly known that the trachea or bronchus contains a foreign body, the patient will be more likely to recover if trusted to the chance of spontaneous expulsion, than he will be if subjected to operation. If this teaching be accepted, it will be important to determine in what cases bronchotomy should be performed, and in what cases avoided. The cases presented will offer some assistance in the solution of the question.

When the nature of the foreign body is known, the propriety of an operation can be more easily determined than when it is not; as it is apparently settled that certain substances are much more likely to be expelled spontaneously than others.

Among the great variety of substances that sometimes find a lodgment in the air-passages, watermelon-seeds—after grains of corn—are the most frequent. Yet the table shows that in the cases in which a watermelon-seed was the foreign substance, there was spontaneous expulsion and recovery in 93.33 per cent., or 16.86 per cent. more than in the cases subjected to bronchotomy. When the accident was the result of the introduction of a grain of coffee, the cases trusted to the efforts of nature gave 29.29 per cent. more recoveries than the cases in which bronchotomy was performed. And in the cases in which seeds of different kinds and miscellaneous substances found a lodgment in the air-passages, a percentage is found in favor of non-interference. The deduction from these facts is, that when the foreign body is one of the kind mentioned, nature will effect more cures than the surgeon. This is the general deduction; but cases will frequently occur in which an operation is imperatively demanded, whatever the foreign substance may be. The conditions demanding a variation from the general rule will be noticed presently.

When the foreign body is impacted in one of the bronchia, the chances of expulsion at the time of operation are small, and attempts at extraction by instruments generally end in failure.

93 deaths are reported after bronchotomy in the table. In 73 cases, or 78.38 per cent., the foreign body was not removed. It is probable that in a large number of these cases the foreign body was impacted in one of the bronchia; it is evident, therefore, that an operation undertaken in such a condition offers but little chance of success. No argument is needed to prove that when a foreign body is so impacted in the trachea, or one of the bronchia, as to make its removal impossible, the addition of a serious wound of the larynx or trachea must add to the patient's danger. Yet one source of increased danger may be mentioned.

In cases where the foreign body is not removed after tracheotomy, death from pneumonia is more likely to occur than in cases without operation. In the cases of death after operation, and without the removal of the foreign body, pneumonia is put down as the cause in 30.13 per cent., while the same disease caused death in only 18.70 per cent. of the fatal cases without operation and without the removal of the foreign body, a difference of 11.43 per cent. It is a legitimate inference in cases of impaction of the kind described, that the chances of life are diminished by bronchotomy. It is admitted that in many cases of impaction the wound in the windpipe has been kept open, and the foreign body expelled after a considerable period of time, but there is an equal number of cases in which no operation was performed, and the same fortunate result occurred. In 5 cases reported in the table, tracheotomy was performed, and several months after the closure of the wound the foreign body was spontaneously expelled. In these cases the danger of a serious operation was added to that which arose from the presence of the foreign body. These patients survived, but how many of the 73 patients reported as dying after operation, and without expulsion of the foreign body, succumbed because of the operation, is a question that can never be answered. In a number of cases an operation was strongly advised, but declined, and recovery followed spontaneous expulsion; in two cases an operation was actually commenced, but abandoned because of hemorrhage, yet the patients recovered.

In many cases the foreign body—after the first paroxysm of

strangulation has passed—causes but little if any trouble, even when it remains for a long period. While we cannot be certain that these patients are ever free from danger while the foreign body is retained, it can be said, basing the assertion upon the results here presented, that non-interference affords a better chance of recovery than bronchotomy.

When non-interference is spoken of, I refer to the operation of bronchotomy. In some cases the foreign body may be removed by other means. Forceps may be used. The laryngoscope, in certain instances, will enable the surgeon to use this instrument with precision and success. In every case that offers a reasonable prospect of success, an effort at direct extraction should be made. The 93 cases here reported, in which the foreign body was thus removed, are evidence that success may sometimes be expected.

Although cases are reported in the table in which expulsion of a foreign body was effected during the acts of sneezing or vomiting, my investigations do not lead me to hold errhines and emetics in higher esteem than do most surgeons of the present day. Indeed it is my conviction that they should never be employed. In regard to inversion of the body and succussion, I hold the same opinion expressed in relation to the use of emetics. Although success sometimes attends their employment, as shown by the 26 cases here reported, the danger of causing immediate death is so great as to overshadow the occasional success reported; I am, therefore, fully in accord with the surgeons who teach that these means should never be employed unless the surgeon is ready to open the windpipe at once should an emergency occur. It would be a still better teaching, I believe, to say that these methods should *never* be resorted to until after an opening into the windpipe has been made.

These observations in relation to other means than bronchotomy for the removal of foreign bodies from the air-passages, are hardly relevant to the subject under consideration. Their importance, however, permits their introduction here.

Having attempted to show why bronchotomy should not be resorted to in many cases where an extraneous substance has

been introduced into the air-passages, it remains to notice the cases in which a resort should be had to the knife. As it very rarely happens that the surgeon sees a case of this accident during the paroxysm of strangulation, that immediately follows the introduction of a foreign body into the air-passages, the question of operation is usually presented at a later period.

Whenever the symptoms continue urgent, or attacks of threatened suffocation come on frequently, bronchotomy should be resorted to without unnecessary delay, provided that direct extraction is not practicable. When threatening symptoms are continuous it will generally be found that the foreign body is lodged in the larynx, and is causing signs of rapid strangulation by reflex action, or a constantly increasing embarrassment of respiration by stenosis depending on œdema, or active inflammation. In such cases the sooner an operation is performed the better. Of the two conditions mentioned as causing constant serious symptoms, the second is the most dangerous, although there may be no paroxysms of strangulation after the first.

In such cases there can hardly be a hope of spontaneous expulsion. I have lately seen a child die in this way. While the child was at play with a piece of dried apple in its mouth, a sudden fit of strangulation came on that lasted only for a short time. An hour afterward, when I saw the child, there was no distress and no symptoms indicating a serious accident beyond a slight embarrassment of respiration and huskiness of the voice. I advised that no operation be performed before the advent of other symptoms. No more paroxysms of strangulation appeared, but the difficulty of respiration slowly increased for two days. Then bronchotomy was proposed, but declined. The symptoms steadily grew worse, and the patient slowly died from asphyxia at the end of the fourth day after the accident. On examination after death a small thin piece of the coriaceous seed-envelope of an apple was found in the right ventricle of the larynx. The mucous membrane of the larynx, and the tissues beneath were highly œdematous. While it is hardly possible that this foreign body could have been directly removed by an operation—its discovery, even at the autopsy, being difficult, owing to its color,

small size, and position—life might have been prolonged by an opening below the seat of obstruction, and a chance been thus gained of spontaneous expulsion at a later period.

A similar condition of the larynx may be present even when the foreign body is lodged in the trachea or bronchia. I have seen an excellent example of this kind while writing these pages.

A boy, four years old, while playing with grains of corn (*maize*) in his mouth, suddenly strangled. The urgent symptoms lasted for some time. After they had subsided, the child was brought a distance of several miles to me. When I saw him the respiration was not greatly embarrassed. The grain of corn was evidently lodged in the right bronchus. I advised delay and heard no more of the case for three days. Then I was sent for and found the conditions greatly changed. The foreign body still remained in the bronchus, interfering much with the passage of air into the right lung. All the signs of slow asphyxia were present, depending on obstruction in the larynx. No paroxysm of strangulation had occurred since the first. I made the operation of laryngo-tracheotomy without further delay. The grain of corn was not dislodged from its position. An instrument was passed through the wound into the mouth, on the supposition that a second grain of corn might be in the larynx. None was found. After the operation the respiration was free and easy. The wound was kept open, and twenty-four hours after the operation, the foreign body escaped through the wound. Several days elapsed before the obstruction in the larynx passed away. The cause of this stenosis I do not know. It may have been the introduction of the father's finger into the throat to provoke vomiting at the time of the accident. However produced, it was this condition that made the operation absolutely necessary, and not the grain of corn impacted in the bronchus.

When the foreign body is loose in the trachea its movements cause frequent attacks of strangulation. In such a case bronchotomy is demanded, not only to afford present relief, but to obviate the great danger of sudden death from a lodgment of the extraneous body in the rima glottidis.

I am aware that statistical evidence is not always reliable,

that in surgical practice it can do no more than indicate in any given case what is probably the correct course of action. But, as the surgical rule here examined has been established on this kind of evidence, it is surely admissible to ask for a reconsideration of the matter in order that new testimony of the same character may be introduced and have its proper influence in determining if the old decision shall be re-affirmed or reversed.

The Fellows of this Association will place, I am sure, a correct value on the new material here presented; and if they are convinced that in these investigations I have sought only the truth, I shall be rewarded for the no small amount of labor expended in collecting and arranging the cases reported, cases that in my opinion justify the following conclusions, viz:—

1. When a foreign body is lodged either in the larynx, trachea, or bronchia, the use of emetics, errhines, or similar means should not be employed, as they increase the sufferings of the patient, and do not increase his chances of recovery.

2. Inversion of the body and succussion are dangerous, and should not be practised unless the windpipe has been previously opened.

3. The presence simply of a foreign body in the larynx, trachea, or bronchia does not make bronchotomy necessary.

4. While a foreign body causes no dangerous symptoms, bronchotomy should not be performed.

5. While a foreign body remains fixed in the trachea or bronchia, as a general rule, bronchotomy should not be practised.

6. When symptoms of suffocation are present, or occur at frequent intervals, bronchotomy should be resorted to without delay.

7. When the foreign body is lodged in the larynx, there being no paroxysms of strangulation, but an increasing difficulty of respiration from œdema or inflammation, bronchotomy is demanded.

8. When the foreign body is movable in the trachea, and excites frequent attacks of strangulation, bronchotomy should be performed.

Foreign Bodies in the Air-Passages.

Cases without operation.	No. and Sex.			Recoveries and Deaths.				Age of Patients, Recoveries.							Age of Patients, Deaths.						
	Total number.	Males.	Females.	Recoveries.	Deaths.	Per cent. of recoveries.	Per cent. of deaths.	Under 1 year.	1 to 2 years.	2 to 3 years.	3 to 4 years.	4 to 5 years.	5 to 10 years.	Over 10 years.	Under 1 year.	1 to 2 years.	2 to 3 years.	3 to 4 years.	4 to 5 years.	5 to 10 years.	Over 10 years.
Total No. of cases	599	375	224	460	139	76.79	23.20	10	52	45	45	34	124	150	7	30	24	15	12	23	28
Grains of corn	92	60	32	66	26	71.74	28.26	2	9	9	6	9	19	12	3	5	7	2	5	4	0
Watermelon seeds	75	39	36	70	5	93.33	6.66	2	7	11	13	4	21	12	0	3	0	1	0	1	0
Beans	51	29	22	30	21	58.82	41.17	0	1	3	5	4	15	2	0	6	8	3	1	2	1
Grains of coffee	34	20	14	29	5	85.29	14.71	2	7	5	6	1	6	2	0	1	1	1	0	1	1
Seeds, various kinds	58	27	31	47	11	81.03	18.96	1	8	5	7	2	13	11	1	2	4	1	0	1	2
Cockleburs	26	18	8	19	7	73.08	26.92	0	0	0	1	0	4	14	0	0	0	0	0	2	5
Miscellaneous	263	182	81	199	64	75.67	24.33	3	20	12	7	14	46	97	3	13	4	7	6	12	19

Cases without operation.	Period of Expulsion.											How Expelled.						Cause of Death.						Published or not.	
	Under 1 day.	1 to 2 days.	2 to 3 days.	3 to 4 days.	4 to 5 days.	5 to 6 days.	6 to 7 days.	1 to 2 weeks.	2 to 3 weeks.	3 to 4 weeks.	Over 1 month.	Coughing.	Vomiting.	Sneezing.	By fall.	Body inverted.	Not stated.	Asphyxia.	Pneumonia.	Exhaustion.	Bronchopneumonia.	Abscess.	Laryngitis.	Consumption.	Published.	Not published.
Total No. of cases	63	26	23	18	12	11	12	60	27	24	184	412	26	5	5	6	6	84	26	4	7	10	5	3	27	572
Grains of corn	6	6	2	3	2	1	0	13	5	8	20	59	7	0	0	0	0	15	5	1	5	0	0	0	2	90
Watermelon seeds	6	2	4	2	2	2	1	11	5	3	32	62	4	2	2	0	0	4	1	0	0	0	0	0	0	75
Beans	5	2	1	1	1	2	1	7	3	1	6	29	1	0	0	0	0	17	3	1	0	0	0	0	2	49
Grains of coffee	3	1	2	2	0	0	2	7	1	2	9	24	3	1	0	0	1	2	3	0	0	0	0	0	1	33
Seeds, various kinds	6	1	3	4	0	2	1	5	2	3	20	40	1	0	3	2	1	6	2	1	0	1	1	0	3	55
Cockleburs	0	0	3	1	1	1	0	3	0	0	7	19	0	0	0	0	0	2	3	0	0	1	0	1	1	25
Miscellaneous	37	14	8	5	6	3	7	14	3	7	90	179	10	2	0	4	4	38	9	1	2	8	4	2	18	245

FOREIGN BODIES IN THE AIR-PASSAGES.—*Continued.*

Cases of operation.	No. and Sex.			Recoveries and Deaths.				Age of Patients, Recoveries							Age of Patients, Deaths.						
	Total number.	Males.	Females.	Recoveries.	Deaths.	Per cent. of recoveries.	Per cent. of deaths.	Under 1 year.	1 to 2 years.	2 to 3 years.	3 to 4 years.	4 to 5 years.	5 to 10 years.	Over years.	Under 1 year.	1 to 2 years.	2 to 3 years.	3 to 4 years.	4 to 5 years.	5 to 10 years.	Over 10 years.
Total No. of cases	338	192	142	245	93	72.48	27.42	12	40	35	22	16	68	43	12	31	15	8	9	14	4
Grains of corn	85	53	31	66	19	77.64	22.36	6	13	10	8	8	13	8	2	7	3	1	3	3	0
Watermelon seeds	34	20	14	26	8	76.47	23.53	0	8	6	2	0	5	2	5	3	0	0	0	0	0
Beans	39	17	22	24	15	61.54	38.46	0	3	5	5	1	6	4	0	7	3	2	1	2	0
Grains of coffee	25	13	12	14	11	56—	44—	5	5	0	1	0	2	1	0	1	3	4	3	0	0
Seeds, various kinds	36	26	9	27	9	75—	25—	1	0	5	2	3	12	3	0	4	0	0	1	4	0
Cockleburs	11	7	4	11	0	100—	0	0	0	1	0	0	1	9	0	0	0	0	0	0	0
Miscellaneous	108	56	50	77	31	71.29	28.71	0	11	8	4	4	29	16	5	9	6	1	1	5	4
	Sex not stated, 4.							Age not stated, 9.													

Cases of operation.	Laryngotomy.					Laryngo-Tracheotomy.					Tracheotomy.					Foreign Body Remov'd or not.		Cause of Death.					Voice Lost or Impaired.		Published or not.	
	Total.	Recovered.	Died.	Per cent. of recoveries.	Per cent of deaths.	Total.	Recovered.	Died.	Per cent. of recoveries.	Per cent. of deaths.	Total.	Recovered.	Died.	Per cent. of recoveries.	Per cent. of deaths.	Removed.	Not removed.	Asphyxia.	Hemorrhage Exhaustion.	Pneumonia.	Other causes.	Lost.	Impaired.	Published.	Not published.
Total No. of cases	36	30	6	83.33	16.66	26	19	7	73.07	26.93	276	196	80	71.02	28.98	20	73	19	8	15	32	19	5	19	71	267
Grains of corn	6	6	0	100—	0	4	3	1	75—	25—	75	57	18	76—	24—	6	15	1	3	4	8	3	1	2	11	74
Watermelon seeds	9	8	1	88.88	11.12	2	1	1	50—	50—	23	17	6	73.91	26.09	0	8	3	1	0	4	0	1	3	7	27
Beans	2	0	2	0	100—	2	2	0	100—	0	35	22	13	62.85	37.15	0	15	4	2	3	1	5	0	1	0	33
Grains of coffee	2	0	2	0	100—	2	1	1	50—	50—	21	13	8	61.91	38.09	5	6	2	0	1	4	4	0	0	2	23
Seeds, various kinds	1	1	0	100—	0	1	0	1	0	100—	33	25	8	75.75	24.25	1	8	4	0	2	2	1	0	0	11	25
Cockleburs	3	3	0	100—	0	0	0	0	0	0	8	8	0	100—	0	0	0	0	0	0	0	0	0	1	0	11
Miscellaneous	12	11	1	91.66	8.34	15	12	3	80—	20—	81	54	27	66.66	33.34	8	23	5	2	5	13	6	3	12	34	74

Foreign Bodies in the Air-Passages.—*Concluded.*

Removal by operation other than Bronchotomy.	No. and Sex.			Recoveries and Deaths.	
	Total number.	Males.	Females.	Recovered.	Died.
Total number of cases	63	40	23	62	1

Removal by operation other than Bronchotomy.	How soon Removed.										
	Under 1 day.	1 to 2 days.	2 to 3 days.	3 to 4 days.	4 to 5 days.	5 to 6 days.	6 to 7 days.	1 to 2 weeks.	2 to 3 weeks.	3 to 4 weeks.	Over 1 Month.
Total number of cases	39	7	4	2	0	0	1	3	0	1	2

Not stated 4.

Removal by operation other than Bronchotomy.	How Removed.					Where Located.			Published or not.	
	Forceps and laryngeal mirror.	Forceps.	Probang.	Wire hook.	Finger.	Larynx.	Trachea.	Not stated.	Published.	Not published.
Total number of cases	28	20	2	3	8	39	3	21	5	58

Not stated 2.

Total number of cases not published	897
Total number of cases published	103
Grand total	1000

Note.—With this paper was presented properly classified tables containing all the material facts in relation to the 1000 cases—as the name, age, sex, and residence of each patient, together with the name and residence of the physician or surgeon reporting the case; the kind of foreign body, how long retained, how expelled, the kind of operation performed, if any, and by whom, with notes on cases of special interest. The great bulk of these tables makes their publication in this volume impossible. They therefore have been bound and permanently deposited in the library of the College of Physicians, Philadelphia, where they may be consulted by those interested.

DISCUSSION.

Dr. H. F. Campbell, of Augusta, Georgia.

I have had four or five cases of tracheotomy. In three of them the statement was confirmed that grains of corn are the most frequent foreign bodies. The other of my cases was one in which the foreign body was a watermelon-seed. The parents refused an operation, and the child died of pneumonia. In the three cases in which I operated, two recovered. The first was a child two years of age. The grain of corn was removed at the time of operation. In the second case, the grain of corn came away on the second day after the operation, while the child was lying in bed. Both of these cases recovered. My third case, which would appear to confirm the opinion that we should not be too ready to operate for a movable foreign body in the trachea, was a boy eight or ten years old. The grain of corn had lodged in the larynx, and was movable. The patient was the child of a physician, who appealed to me to perform tracheotomy. I was guided by the rule that a foreign body in the larynx must be removed. There was considerable bleeding at the time of the operation, still, all due diligence was practised. The foreign body was not removed. After controlling the hemorrhage I left the boy in the care of his father, the grain of corn still moving during the acts of respiration, and appearing at the opening. In order to favor expulsion of the foreign body, I inverted the child. I discovered that this produced great turgescence of the vessels of the neck and caused bleeding; I therefore desisted. During an attack of suffocation, which the father attempted to relieve by inverting the child, blood immediately began to flow, and death ensued. The bleeding was not sufficient in itself to cause death; combined with the impeded respiration it caused a fatal result. These are the results of my three operations.

One of the most interesting cases of tracheotomy which I have seen was that performed for the removal of a nail which had become lodged, head upwards, in the left bronchus. In order to remove it, an opening was made in the trachea, a pair of long forceps was introduced, and the nail extracted through the opening.

Dr. J. Ewing Mears, of Philadelphia, Pennsylvania.

I think it proper to say that this Association should feel very much indebted to Dr. Weist for the great care which he has taken, and the great labor which he has expended, in the preparation of his paper. It is certainly a valuable contribution to the subject, and the conclusions presented will, I believe, be accepted as correct.

I can add two cases to those which have been reported by him. In the first case the foreign body, a piece of sponge, lodged in the larynx while I was performing the operation of excision of the tongue. As an assistant was passing a sponge into the mouth and pharynx, the sponge became detached from the holder, and was lodged in the larynx. The symptoms of asphyxia at once supervened, and tracheotomy was immediately performed, not by any careful dissection, but by plunging the knife directly into the trachea. The sponge was then removed, by pushing it up into the pharynx. The patient died, but I cannot say that the tracheotomy which was performed for the removal of the sponge contributed to the result.

I recall to my mind a very interesting case, which was reported some years ago by a physician of Philadelphia. A boy, eleven or twelve years old, presented grave symptoms of pulmonary disease, and was rapidly declining in health. His parents, in order to give him an afternoon of recreation, took him to a pleasure garden near the city. While there he fell out of a swing, striking on his head, and there was found upon the ground at that spot a penny, which had been expelled from the bronchus. The pulmonary symptoms disappeared, and the child's health was rapidly restored.

Dr. C. B. Nancrede, of Philadelphia, Pennsylvania.

I would like to ask several questions. In considering the greater mortality of operative interference over letting cases alone, I would ask: May not error arise from the pre-existent broncho-pneumonia set up by the foreign body; are not those cases only operated upon where recovery has not followed the letting-alone treatment; and where prior inflammatory troubles have been set up, has not the fatality of the operation been due to these complications? We know how fatal tracheotomy was in pseudo-membranous laryngitis, chiefly from avoidable complications, before the introduction of some means by which the dust in the air could be kept from the air-passages, and also by which the natural moistening of the air could

be accomplished. Now, by the use of gauze—not carbolized—and by moistening the air, we all know that the success is much greater, thereby proving that the operation *itself* is not a source of much danger. I think that no one who has once performed the operation is desirous of doing it again unnecessarily. I have performed it a number of times, four times already this year, for laryngeal obstruction, but I hope never to have to do it again.

Dr. Weist. I shall occupy very little time. As a general rule, I value my papers precisely in proportion to the amount of discussion they provoke, and in proportion to the amount of opposition they meet. But I do not feel that the paper I have presented is of no value because of the small amount of discussion it has elicited. The small amount of discussion elicited is due, I think, to the fact that I have presented such a mass of new testimony that my conclusions cannot be disputed. I do not want to flatter myself, but I have attacked a rule which is laid down in every surgical work that I know of. I come here with nearly 900 new cases, in order to test whether or not that rule is still correct. I think my facts and conclusions have not been disputed, because they have been presented in such a way as to show that the testimony in favor of non-interference in certain cases overbalances that in favor of the old surgical views. If this is true, I am satisfied that my labor has been well spent. I have repeatedly performed this operation for foreign bodies in the air-passages and for croup, but I should be glad if I never had to perform the operation again.

In answer to the questions propounded: I would say that if cases were operated upon immediately, or very soon after the introduction of the foreign body, before the occurrence of inflammation, statistics would undoubtedly show a larger percentage of recoveries than is found in my collection, which is made up of cases operated upon at varying times after the accident. But any error that may arise in the conclusion from this is certainly offset by the fact that, in patients in whom spontaneous expulsion occurred, the average time before expulsion would greatly exceed the average time before the operation was performed. Cases have been reported in which twenty years have elapsed. The average is what I have presented.

THE ANTISEPTIC TREATMENT OF WOUNDS AFTER OPERATIONS AND INJURIES.

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In presenting to the distinguished surgeons of this learned body a paper upon the "Antiseptic Treatment of Wounds after Operations and Injuries," I feel profoundly conscious of my inability to do justice to a subject, the magnitude and importance of which is universally conceded.

I have nothing new to offer for your consideration in the treatment of wounds, and have obtained my own consent to undertake the task assigned me, solely from an earnest desire to elicit discussion, to call forth the wealth of knowledge possessed by the members of this Association, and to invite comparison of the results of treatment obtained by representative surgeons of the American profession.

Before proceeding to the discussion of the subject, I wish to clearly define the position I have taken. No single method of wound treatment can constitute the antiseptic method *par excellence*, to the exclusion of others. As a distinguished surgeon in the great debate at the International Medical Congress of 1881, pithily said: "All good surgery is antiseptic, would that I could say also, that all antiseptic surgery was good." Unfortunately this axiom is too often disregarded, and enthusiasts are far too prone to pin their faith to what they deem true antiseptic methods, to the neglect of cardinal principles of wound repair. I hold that true antiseptic surgery applies to the employment in the treatment of wounds, of all measures and agents that will most effectually prevent wound disorders, especially putrefaction,

or if they cannot be prevented, that will remove or render nugatory their evil effects.

So general is the practice of antiseptic surgery, and so great the faith of the profession in the necessity of its observance, that bold indeed must be the surgeon, who, in the face of the revolution and experience of the past few years, would deny its virtue or dare neglect its teachings.

While all acknowledge, however, the great value of antiseptic surgery, its practice is of infinite variety. One class of surgeons tries to anticipate and to prevent putrefaction; another attempts to seal the absorbent surface, and thus to shut out its products. One leaves the wound open for the ready escape of the enemy, another closes the path of entrance so that he cannot enter. All, however, seek the same end, namely, to prevent putrescent matters from forming in wounds, or to remove them before they have produced their injurious effects.

Of the different methods of antiseptic wound treatment, none has occupied so prominent a place in the profession as the system founded by Mr. Lister. Only a little more than a decade has passed since he first announced his method of treating wounds upon the antiseptic plan, and yet it has produced a revolution in wound management—it has made an epoch in surgery. The advance of surgery in the last ten years equals what usually requires a century to accomplish. To the enthusiastic labors of Lister more than to those of any one, belong the credit of having put in operation the movement which has led to this most brilliant period in surgical history. Yet, while an almost unanimous verdict in favor of this practice has been expressed, many eminent surgeons in various parts of the world differ in regard to the theoretical points upon which his method is founded and by which its success is sought to be explained.

It has long been known that the admission of air into wounds interfered with the healing process, and various hypothetical views have been from time to time advanced in explanation of its harmful influence. Mr. Lister, reasoning from the proofs afforded by Pasteur's experiments, concluded that the disturbance of the reparative process was due not to the air in its entirety,

but to the microscopic organisms which it contains. Lister's theory, as is well known, is that the bacteria in the air are the cause of putrefaction in wounds, and that when it is possible to exclude all the sources of these living organisms, putrefaction with its train of evil consequences fails to occur, and rapid healing of wounds and injuries proceeds uninterruptedly.

The whole system of antiseptic surgery, according to Lister, is based upon the following postulates:—

1st. That pathogenic germs are always found in the atmosphere.

2d. That these germs, if allowed to come in contact with open wounds, have the power not only of causing putrefaction, but also of entering the tissues around, and, by their rapid increase, of pervading the exposed region and of spreading their evil effects throughout the entire system.

3d. That the entrance of these germs into open wounds is the sole cause of all septic accidents.

It has been proved beyond a doubt that the air does contain germs, together with all sorts of inorganic microscopic particles. Tyndall has rendered these corpuscles visible by passing a ray of light into a darkened chamber. Tissandier has demonstrated their presence in dust. Pasteur has cultivated them in liquids.

If we seek to learn the truth from the investigations of the most distinguished savants who have occupied themselves in the study of these parasites, we are astonished at the poverty of information concerning organisms to which such an important role has been assigned in the causation of disease and wound accidents. The results have been so contradictory and the opinions advanced so conflicting that all knowledge concerning them is extremely hypothetical and uncertain. They are presumed to be of vegetable origin, though it is not definitely settled whether they belong to the class of algæ or fungi. They are the smallest of living things, the most common kind being about one thirty thousandth of an inch in diameter. They present variations in shape and in general appearance; even their origin is still a matter of discussion, some contending that they are generated spontaneously, others that they are derived from individuals like

themselves; while still others believe that they are derived from pre-existing organic matter. M. Onimus has shown that bacteria may be generated in media, which contains albuminoid substances, even when air is excluded. Neusch demonstrated that bacteria are produced in the interior of animal and vegetable cells without any lesion and without coming from the air. Their presence even in the interior of eggs has time and again been verified. The most diverse opinions are held in regard to the classification of bacteria. One of the most distinguished investigators of this subject, M. Cohn, insists upon the impossibility of distinguishing with certainty their genera and species.

The great confusion and doubt that exist concerning atmospheric micro-organisms, is well expressed by Cohn, in the following extract:—

"So long as our microscope makers do not place at our disposal much higher powers, we will find ourselves in the domain of bacteria, in the situation of a traveller who wanders in an unknown country at the hour of twilight, at a moment when the light of day no longer suffices to enable him to distinguish objects, and when he is conscious that notwithstanding all of his precautions, he is liable to lose his way."

The same uncertainty and doubt enshroud the role of bacteria in surgical lesions. Floating freely in the surrounding air, it is not surprising that they should be found on the surface of the body and in the interior of organs communicating with the air; but since they are likewise found in all tissues of the body, they must gain access through the air we breathe, the water we drink, or the food we eat; or else they must originate in the tissues themselves. They are seen upon the surface of wounds, which are, nevertheless, in a perfectly healthy condition. It has been shown that wounds may undergo complete and thorough reparation while covered with bacteria, and that a patient may remain free from fever or other evil effects, when the discharges from such wounds swarm with them. It has also been shown, that the secretions from injuries which healed rapidly without fever under Lister's or Guerin's methods, contained bacteria as

often as the secretions from wounds dressed simply with reference to cleanliness.

Cunningham has shown that bacteria may be present in great numbers in pus taken from wounds, when no fever exists; that their appearance in the wound discharges has no immediate relation to the unfavorable course of an inflammation, and that the pus taken from cavities communicating with the external air, almost constantly contains bacteria, while the patient may remain free from fever and make a good recovery.

Bergeron (Academy of Sciences, 1875) exhibited the results of his examination of the pus from eighteen closed abscesses. He found bacteria in all hot abscesses of adults, but none in cold abscesses, nor in the acute abscesses of patients under twenty-one years of age. The presence of bacteria in these cases was not attended with any serious symptoms.

Virchow states that he was unable to discover any appreciable difference between the pus from wounds healed after Lister's method, and that resulting from ordinary methods. It is now generally conceded, and Mr. Lister himself acknowledges the fact, that bacteria can work no harm to the body while the vital action is maintained in its integrity. It is a question with many pathologists whether the little bodies found in putrescent material in such numbers are bacteria, or whether they are the granules of disintegrated tissue. It is acknowledged by the most expert microscopist, that it is almost impossible to distinguish the granules of disintegrated tissue, especially such as result from the degeneration of white corpuscles, from bacteria.

In a very interesting paper published in the *Liverpool and Manchester Surgical Reports* for 1878, by Dr. James Ross, it is claimed that the so-called bacteria which have been so often said to exist in infectious fluids, are not bacteria, but free granules and portions of protoplasm formed from the disintegration of tissues. In support of this view, he states that in the central contents of an abscess produced by injecting a few drops of strong ammonia into a frog's thigh, he found very few altered red corpuscles, scarcely any full-sized white cells, but a great number of corpuscles varying in size from five-thousandth to

twenty-thousandth of an inch in diameter, which could only be distinguished from bacteria by the application of chemical tests, proofs which have not been furnished by the discoverers of bacteria, so called, in infectious fluids.

Dr. R. R. Gregg, in an able article upon Bacteria in Diptheria, in the *New York Medical Record*, vol. xxi. p. 150, writes: "There is not the slightest difference between the three classified forms of so-called bacteria, viz., spiral, rod-like, and spherical, and the three exactly corresponding forms of coagulated fibrin, viz., granular, thread-like, and spiral. Wood, in his *Practice of Medicine*, when speaking of the organization of inflammatory exudates, says, 'as it escapes it is a homogeneous, transparent fluid, but soon afterward, if examined by a microscope, it is found to contain multitudes of fibrils and a great number of granules of different sizes,' and under circumstances when observers say bacteria are found in greatest abundance, the fibriles and granules of fibrin are found in greatest profusion."

The unprecedented success attending the treatment of wounds after Lister's plan, is the strongest argument in support of the germ theory. If it can be shown, however, that other methods, based upon different principles, can exhibit equally successful results, that fact can no longer carry weight as an argument. The supremacy of Lister's system would be undisputed could it be proved by statistics, that wounds treated upon his plan offered results decidedly superior to such as are obtained by other modes of treatment. Unfortunately for Listerism such superiority cannot be demonstrated, and the present unsettled opinion concerning the proper status of his method is due in a great measure to that fact. Statistical comparison of the Listerian form of treatment with other methods founded upon different views, shows that the results are as good in a large majority of cases in one as in another. Lister himself, with an abiding faith in the germ theory and in the perfectness of his method, claims that success should be the invariable rule, failure the exception, and always attributable to some flaw in the application of the dressing. But in order to secure this measure of success, he insisted at first upon the vital importance of the most rigid observance of every

detail of the complicated dressing. More recently, however, Mr. Lister has been obliged to confess that the requirements of his dressing are not so absolute as he at first imagined, and he now agrees with the majority of surgeons that the carbolized spray should be regarded more in the light of an adjuvant than as an essential. Other surgeons, firm believers in the germ theory, have from time to time left off first one, then another article of the cumbrous dressing, without any perceptible difference in the successful results.

Some other explanation of the success of Lister's method must be sought than that furnished by the germ theory, and there can be no doubt but that a satisfactory explanation can be found for his special method, as well as for all others based on the germ theory.

If in a review of Lister's method, and of the various forms of wound treatment that owe their origin directly or indirectly to his method, we seek to learn what they possess in common, it will be seen that all in one form or another have provided equable temperature, absolute immobility, continuous pressure, and thorough drainage, which either alone or in combination tend to moderate and control inflammation in wounds.

Upon the well-known properties they severally possess of acting antiphlogistically depends their value as elements of wound treatment. Lister, however, insists upon the importance of carbolic acid as an indispensable article of the antiseptic dressing on account of its action as a germ destroyer. Carbolic acid is in fact the keystone of the Listerian wound treatment. Listerism without carbolic acid is not Listerism. That carbolic acid does exercise the most salutary influence over the healing of wounds has long been recognized, and for many years prior to the development of the germ theory, it was in common use in the treatment of wounds for the remarkable power it was known to exert against inflammatory action; but it owes the permanent place it now occupies in Lister's system to its germicidal properties. The question naturally suggests itself whether the beneficial action of carbolic acid is due to its power of controlling inflammation, or to its action as a germicide. Carbolic

acid, though an energetic germicide, is inferior as such to tannin, thymol, salicylic acid, etc.; yet, when these more powerful germicides are substituted in surgical dressings for the weaker, the beneficial effects are not near so manifest. This fact alone goes far to show that carbolic acid exerts an action over and beyond that of a germicide. It is well known that when carbolic acid is applied to the unbroken cuticle, it produces a benumbing or anæsthetizing effect. May it not be that this action, rendered more intense by the agent being brought in contact with an open wound, so deadens and paralyzes the sentient nerve fibres as to lessen the susceptibility of the part to irritation and subsequent inflammation? Carbolic acid modifies vital action, and in this way serves to control inflammatory over-action. This view of its action upon wounds, would, in my opinion, satisfactorily explain the success of Lister's treatment, and the explanation is even more satisfactory than it could possibly be upon the grounds of the germ theory. The action of all antiseptic medicinal agents may be accounted for in the same way. Iodoform, which is now enjoying a reputation, second only to that of carbolic acid as an antiseptic, owes its influence over wound repair to a similar power. I dare say if there could be found an agent that possessed equal antiphlogistic powers with carbolic acid, without any germ-destroying power, and this agent was substituted for carbolic acid in the Listerian dressing, the result would be equally favorable.

The germ theory is at fault and furnishes a very unstable foundation for a system of wound treatment. I do not wish to be understood as denying *in toto* the germ of wound disorders. I can readily conceive how germs, dust, lint, pollen, and other substances floating in the air, may be like instruments, sponges and articles of dressing, the carriers of a chemical ferment derived from putrescent albuminoid substances, which, by direct contact, produce catalytic changes in the tissues independent of bacteria; but I do emphatically dissent from the views of Lister that bacteria are invariably to be held responsible for all wound accidents, and that all treatment should be directed to opposing their entrance into wounds or to neutralizing their effects.

Until quite recently, the profession all over the world was too much disposed to regard Listerism and antiseptic surgery as synonymous terms. Surgeons, ever on the alert for the discovery of means of preventing septicæmia, pyæmia, erysipelas, and other evil consequences of wound infection, eagerly accepted a special method, the successful application of which promised so much, regardless of theoretical objections, and forgetful to a great extent of the fundamental laws that govern physiological repair. Within the past few years, however, a reaction has taken place, and now the majority of surgeons, while fully recognizing the benefits of Lister's method, have come to regard it as but one of a number of important elements in the surgery of wounds.

Antiseptic surgery does not depend upon the recognition of the germ theory, nor indeed can it be limited to any single rule or set of rules. Gamgee's celebrated plan of treating wounds by pressure, drainage, dry and infrequent dressings, has as strong claims to being considered an application of antiseptic surgery as the method of Mr. Lister. Wood's open treatment of wounds, Callender's well-known procedure, Esmarch's permanent dressing, and, indeed, all methods that are intended to promote wound repair and to diminish wound accidents to a minimum, may properly be considered as examples of antiseptic surgery. In my opinion, then, antiseptic surgery is the rational treatment of wounds, based upon a thorough knowledge of the conditions necessary to healthy repair, and upon a full appreciation of the conditions which threaten to work or are working disorders in them.

It is a generally accepted fact that septic accidents are due to putrefactive decomposition of the organic products of wounds. Concerning the nature of this process, opinions are at variance; some authorities asserting that it represents essential chemical changes, others that it can only occur in the presence and under the influence of vegetable organisms. To this latter belief the advocates of the germ theory adhere. I incline to the opinion that the process is as purely chemical and as fully independent of organisms, as are certain changes brought about in the body by the presence of the various physiological ferments. Putrefac-

tion is the decomposition of animal matter deprived of life and placed under special circumstances. When a portion of the body is deprived of the controlling influence of vitality, chemical changes are allowed full sway, a retrograde metamorphosis of nitrogenous substances takes place by the destruction of the chemical affinities which held the molecules of that compound together, and a re-arrangement of the molecules in a new product ensues.

Billroth says, "I think a substance may be formed as the last action of a dying tissue that shall have some of the peculiarities of a ferment and be very poisonous to the circulating blood. It does not seem to have been proved that the addition of small organisms (vibriones and bacteria of Pasteur) is absolutely necessary to the formation of such a substance."

While it is true, that in all putrescent material bacteria are found in great numbers, it is still a question whether their presence is the cause or the effect of putrefaction. It is highly probable, that putrefying matters form a nidus for the rapid growth and development of bacteria, and that they exist merely as accidental concomitants. A fact that carries great weight as an argument in opposition to the bacterian origin of putrefaction, is that it may occur in parts with which the germ-laden air does not come in contact.

In obedience to one of the universal laws of nature, all organic matter deprived of life manifests a tendency to undergo this retrograde metamorphosis. Therefore, vitality, the great antiseptic, is the enemy of putrefaction, and such measures as maintain the vitality of injured parts may be considered as antiseptic.

Antiseptic surgery should be accredited with a much broader significance than has been generally given to it. It embraces all measures and conditions that tend to prevent putrefaction in wounds, or to remove or neutralize the effects of such putrefaction when present. In order that its application to the treatment of wounds and injuries may be attended with the best results, it is highly essential that such a comprehensive view should be taken of the physiological and pathological processes of repair in wounds, as will enable the surgeon to appropriate and make use

of all the therapeutic agents at his command with judgment and discretion. No one can doubt that rapid and healthy repair offers the strongest safeguard against putrefaction and septic complications, and therefore that all measures that act to promote early union of wounds tend most effectually to prevent the development of such complications. Under favorable circumstances, nature is fully capable of healing solutions of continuity with but little aid from the surgeon, and often with so little disturbance that the manner of healing escapes observation.

Wounds are always healed through the agency of reparative inflammation. It is a universal law that every lesion of tissue calls forth an appropriate and usually a sufficient amount of reparative action, the response to which is increased afflux of blood, exaggerated cell proliferation and tissue formation, phenomena characteristic of inflammation. However slight and insignificant the breach, however favorable the conditions for repair, inflammation is invariably the essential elements in its healing. Through its agency nutritive material is supplied to the injured part. In those wounds which unite without suppuration, the supply of reparative material exactly equals the demand. Hence it is, that in a simple incised wound in which healing by the first intention takes place, the process of repair by inflammation is of a constructive kind, and of such a low grade as hardly to constitute an appreciable change from normal textural activity. Indeed, some deny the presence of any inflammation whatever in wounds which heal by direct union. More frequently, however, the resulting inflammation is of a higher degree. The wound becomes swollen, hot, red, and painful, and the grade of its intensity may be approximately measured by the acuteness and continuance of these symptoms. When this form of inflammation, whether primarily or secondarily induced, takes place, the healing pabulum is furnished in excess of the demand, and the superfluity is converted into pus and as such thrown off. Pus, therefore, may be regarded as the degeneration of the inflammatory product, which is over and beyond the necessities of repair. Its presence betokens a higher grade of inflammation, which is always of more or less destructive char-

acter. When suppuration occurs, molecular death to a greater or less extent must result, and therefore such inflammation not only retards healing, but also threatens to lead to putrefactive changes and septic accidents. Were it possible by treatment to so gauge the intensity of inflammation in wounds, that no higher action would supervene than would be absolutely necessary for reparative purposes, not only would there be a diminished amount of wound products to undergo decomposition, but there would never be any formation of pus. There are, however, so many causes, local and general, to interfere with reparation that very often the constructive is changed into destructive inflammation. The morbid action becomes so intense, the amount of nutritive material is sent to the injured part in such large quantities and in such disorder as to destroy the vitality of the part. The nutritive act is in such instances overwhelmed and destruction seizes upon not only the material intended for reparative purposes, but also upon the tissues themselves. Disintegration and putrefaction rapidily ensue, causing, if the products so changed are not speedily removed or neutralized, serious obstacles to repair, either locally by aggravating the inflammation, or generally by entering the circulation and giving rise to septic poisoning of the system.

It is, therefore, but rational to formulate the concluding proposition of this paper into the following: The antiseptic treatment of wounds consists first, of such means as will restrain inflammatory action within reparative bounds; and second, of such means as will subdue excessive action and remove or neutralize the effects of destructive inflammation. To ascertain the best means of attaining these ends, material assistance is afforded by a careful study of the conditions that most frequently convert physiological action into pathological over action, and of the methods, now in practice among surgeons, that best prevent the development of those conditions.

The causes that may give rise to destructive inflammation in wounds are of infinite number and variety, but they are too familiar to the surgeon to require especial elaboration in a paper of this character. The influence of a vitiated state of the blood

from disease or dyscrasia, of the two extremes of life, of certain seasons of the year, of altered nerve supply, of unfavorable hygienic surrounding, etc., in predisposing wounds to destructive inflammation is generally recognized.

The local factors that are active in causing inflammation to transcend the bounds of a simple process, are foreign bodies, the extremes of heat and cold, mechanical and chemical violence, the character of the wound, etc. These conditions tend to invite an excess of inflammatory action in wounds, by over stimulating the healing forces. The surgeon must ever be on his guard in order to protect them from such influences. Dangers menace from every quarter, and it behooves him in treating wounds, like a skilful general, to so dispose his forces as to be fully prepared to meet attacks from every side.

How, then, may the surgeon prevent the access of destructive inflammation to wounds? How may he control the process so far, at least, that the resulting harm may be reduced to a minimum? To confine the act of healing in all wounds to that of a simple reparative process, is impossible. The nature of many wounds precludes the idea of preventing the onset of a certain amount of destructive inflammation. To treat wounds of every kind after the same method would be as absurd as to treat every disease with the same medicine. Every wound, like every disease, must be treated upon its own merits. The course of treatment that proves successful in some wounds may result disastrously in others. Therefore, in the discussion of the antiseptic treatment of wounds after operations and injuries, it will be well to remember that the treatment of wounds after operations and that of wounds accidentally produced, is materially different. There are two distinct classes of injuries that require distinct methods of treatment. In the one, of which wounds after operations form a large majority, healing by reparative inflammation may be reasonably expected; in the other, the process of repair is necessarily interfered with by a greater or less amount of destructive inflammation. Those made by design or accident, that partake of the nature of incised wounds, belong to one; those in which the vitality of the tissues are seriously

compromised, are included in the other. In the first class septic accidents may be avoided by securing rapid union; in the second class, the same object may be effected by controlling the destructive inflammation that necessarily supervenes, and by facilitating the removal of substances that are liable to become foci of putrefaction.

In attempting to fulfil the indications of treatment in the first class of wounds, the surgeon should fully appreciate the great importance of having the system in the highest state of vital resistance at the time of the operation, and should subject no patient to a capital operation without having submitted him to a judicious course of preparatory treatment. When the vascular, nervous, and organic systems are in a state of health, the vital resistance of the tissues is at the maximum, and wounds made under such circumstances heal rapidly with an action but little removed from that of the nutritive process. The marvellous success which Dudley, the great American lithotomist, obtained not only in the removal of stone from the bladder, but in all the capital operations, was due in a great measure to the careful preparatory treatment to which all of his patients were submitted. Every portion of the body should receive careful attention, every weak point in the economy sought out and by proper means strengthened. The surgeon should aim to procure for his patients what has been termed a preseptic condition.

In cases of emergency when circumstances do not permit the necessary preparation, the treatment after operations should be characterized by marked attention to the surroundings of the patient, and additional care should be taken to secure the healthy action of all parts of the body. Every one can recall instances in which from a depraved state of the system, disastrous results have followed the simplest operations in surgery.

Having procured, if possible, a preseptic condition of the system, the local management of this class of wounds is very simple. There is certainly no necessity of a complicated and expensive system of dressing. "Nature," says Paracelsus, "is the physician of wounds." All that nature demands for safe and speedy repair, is that the wound should be placed in a condition that

will allow her to do her work without interruption, and the province of the surgeon is simply to observe and execute the laws of repair.

It is very important to check immediate and prevent subsequent hemorrhage by securing every bleeding vessel with the animal ligature. All coagula should be removed from the cut surface, for their presence not only interferes with healing by separating the sides of the wound, but acting as foreign bodies they favor the development of excessive inflammation.

The wound should be freed from all foreign bodies, and its surfaces thoroughly cleansed with a solution of carbolic or boracic acid. The hands of the operator, instruments, sponges, etc., should also be thoroughly cleansed, in order that all possible sources of infection from them may be avoided. Cleanliness, however, is a term often misunderstood. It does not refer simply to the removal of dirt (earth), but to the removal of all substances, that by their presence interfere with the healing process, and that by their liability to undergo putrefaction, add to the risks of septic infection.

Accurate apposition, not only of the edges but of the sides of the wound, should be effected and maintained by appropriate means. When sutures are used, tension should be avoided, for tension whether produced from undue tightness of the sutures, or from the accumulation of fluids, militates seriously against repair, by provoking excessive inflammation.

No element of wound treatment is more potent in assisting nature in her efforts to effect safe and rapid repair than thorough drainage, first by avoiding tension, which is so injurious to the healing process; secondly, by preventing the accumulation of fluid products which will undergo putrefaction and poison the system by absorption. The dressing of wounds should be so arranged as to allow the free escape of the fluids from their surfaces, and if the nature of the wound renders it difficult for the fluids to escape, drainage-tubes should be inserted in sufficient number to effect the purpose.

The beneficial effects of moderate and uniform temperature in the reparation of wounds is strongly exemplified in the healing

of subcutaneous wounds. Guerin's cotton-wool dressing, by maintaining equable temperature in wounds, has obtained an enviable reputation. A material should be chosen as a protective covering that will shield wounds from changes of temperature, without producing over-heat, and that will at the same time absorb their discharges.

The value of compression in controlling the circulation in parts of the body, and thereby preventing excessive inflammation, is fully recognized. Methodical pressure, made by roller bandages, or by adhesive plasters, applied over absorbent cotton, are invaluable adjuvants in the treatment of wounds.

The position of a wounded part has an important bearing upon the rapidity of its healing. That position should be chosen which will best serve to diminish the amount of blood circulating in the part.

The value of rest in the treatment of wounds cannot be overestimated. Motion is a fruitful source of inflammatory complications. Not only is repair interfered with by the breaking up of the delicate bonds of new formation, but the danger of septic infection is greatly increased by the production of rents in the embryonic tissues, which permit the entrance into the circulation of putrid matters. Therefore, the parts should be immobilized as much as possible, and the dressing of the wound made at as long intervals as circumstances will permit. Nothing is so productive of harm as meddlesome surgery. One of the principal elements of success of Gamgee's, Esmarch's, and, indeed, of most of the modern methods, is the infrequency of dressing.

To recapitulate, the elements that perform an important part in the treatment of this class of wounds, are—

1. Careful preparatory treatment.
2. Judicious after-treatment.
3. Removal of foreign bodies.
4. Perfect cleanliness.
5. Accurate coaptation of the edges and sides of wounds.
6. Thorough drainage.
7. Maintenance of equable temperature.

8. Methodical compression.
9. Position.
10. Rest.

The treatment of the second class of wounds differs very materially from that of the first. The contused and lacerated condition of such wounds, especially those resulting from falls, blows, machinery, railroad accidents, etc., render all attempts to procure union by the first intention futile. The course of repair is necessarily tedious and doubtful, and the dangers of septic complications are greatly increased from the presence of seriously compromised, if not completely devitalized, particles or masses of tissue. The facts, that patients in whom these wounds are most likely to occur are especially predisposed to high grades of inflammation, that the character of these wounds demands greater reparative effort, that more or less tissue death is an invariable concomitant, that in consequence putrefaction is more eminently threatened, are sufficient to call forth the best resources of the surgeon in order to conduct them to a successful termination.

When excessive inflammation has occurred, whether from the character of the wounds or from any other cause, the most active efforts should be made to lessen its intensity as much as possible, and to remove its products before they have undergone putrefactive changes. Constitutional remedies which are known to have power in subduing the inflammatory process, should be employed with judgment and discretion. Care should be taken that rest, position, drainage, etc., are secured, and that all the local means that can favorably influence the course of the morbid action, should be used at the discretion of the surgeon. The open method of treatment, combined with frequent irrigations of warm carbolized water, by which all excess of exudation as well as portions of bruised or otherwise injured tissue may be removed as soon as formed or separated, has been productive of the best results.

If the wounds are closed, it is highly essential that suitable outlets should be provided for the inflammatory products, not only for those which collect soon after the injury, but also for

the later exudative forms. Chassaignac's drainage-tubes are invaluable when properly employed for this purpose. Through these tubes, carbolized water should be frequently injected, so as to thoroughly cleanse the entire surface of the wound. Success in the treatment of this class of wounds may be measured by the surgeon's ability to overcome excessive action, and by his power to prevent or render harmless septic influences.

In considering the various agents that are held in high repute by the profession in the treatment of wounds, it will be noticed that they are all antiphlogistic in their action, and that by virtue of this power they prevent wound accidents and remove their evil effects. They are therefore antiseptics. An antiphlogistic is virtually an antiseptic; they should be considered synonymous terms.

In concluding this paper upon a subject that more than any other is engaging at the present time the earnest study of surgeons all over the world, I would beg to remind the members of this distinguished body, that I have not attempted to offer for their consideration any novel or distinct method of treating wounds. I have made no iconoclastic effort to destroy confidence in methods of wound treatment now in vogue among surgeons, nor have I wished to detract in any way from the great debt the profession and the world owe to Mr. Lister, in the revolution his labors and researches have originated in the treatment of wounds. No one can appreciate more fully than myself the immense value of Mr. Lister's contributions to surgery.

I have in the foregoing remarks simply endeavored to draw your attention to the fact, that antiseptic surgery can be represented *solely*, neither by Mr. Lister's nor by any other method, but that the treatment of wounds must be based upon a firmer, more fully established basis than that which the germ theory affords. I have striven to demonstrate the fact that all good surgery is antiseptic; that surgery, in order to be good, must be directed to the prevention or to the control of inflammation, which is the "fons et origo" of all wound accidents, and that, therefore, the method of wound treatment which most perfectly fulfils these indications may most deservedly claim to be anti-

septic. The points I have endeavored to sustain in this paper may be conveniently formulated into the following propositions:—

1st. The germ theory of wound infection is not established.

2d. The antiseptic treatment of wounds after operations and injuries is not limited to Listerism nor to any other special method, but is based upon broad, general principles.

3d. Antiseptic surgery embraces every condition or agent that tends to prevent putrefactive changes in wounds, or to remove or neutralize the effects of such changes when they have occurred.

4th. All wounds are healed by reparative inflammation.

5th. All wound accidents are the result, either directly or indirectly, of destructive inflammation.

6th. The antiseptic treatment of wounds, properly considered, consists first, of such means as will restrain inflammatory action within reparative bounds; and second, of such means as will subdue excessive action and remove or neutralize the effects of destructive inflammation.

DISCUSSION.

Dr. E. M. Moore, of Rochester, New York.

I think that we shall all agree with the views expressed in Dr. Briggs's paper. I think they are satisfactory in every way. I am making a series of experiments in reference to antiseptic surgery. The great advantage of subcutaneous surgery we all know. The great advantage of getting rid of the air has been repeated many times to-day. The aim I have in view is to give to external surgery some of the advantages of internal surgery. This, of course, is what Mr. Lister has tried to do. He attempts to purify the air which comes in contact with the wound. It seems to me that in a large number of cases we are able to do this more readily than by his method. We are able to exclude the air. We are able to exclude the air when we amputate, or perform an ovariotomy. If we succeed in doing this, I think that antiseptic principles will be more fixed. I have

performed ovariotomy four times in this way. I consider four cases of no value; and, indeed, I lost two of my patients; however, not with peritonitis—the post-mortem examination showed no such condition. I simply pass carbonic acid gas directly into the place where the operation is to be performed, and in consequence of its being heavier than the atmosphere it pre-occupies the space. I have great hopes from this plan.

I had an opportunity once of seeing a little coagulable lymph. I think very few gentlemen have seen coagulable lymph. I had on one occasion a case of compound fracture at the elbow; a small artery was bleeding; I thrust in my finger to stop it; I pushed my finger into the joint; I had reason to believe that I would have suppuration, but I thought it unwise to seek the artery, for then I should have had suppuration and probably anchylosis. Recollecting that the flow of blood, when it is free, is one of the best agents of antiseptic surgery to exclude the air, I simply bound the wound up primarily, with several layers of cloth, for the purpose of preventing hemorrhage by the pressure. No untoward result occurred. At the end of nine days I thought it prudent to look at the wound, as there were no evidences of inflammation. In taking off the different coverings I found them saturated with blood, which had dried, and thus had been the most perfect protection which it was possible to conceive of. This is what Sir Astley Cooper taught us in regard to the knee-joint. After removing the dressing, I found in the bottom of the wound a piece of coagulable lymph as large as a filbert, which was simply lying in the wound.

I speak of this as being one of the means of antiseptic surgery.

Dr. H. F. Campbell, of Augusta, Georgia.

My invariable practice after operations and in injuries, is to administer to the patient daily a sufficient amount of cinchona, in some form, so as to produce cinchonism, and maintain it. This treatment is based upon my theory of suppuration. I do not believe that bacteria or other microscopical organisms are the cause of that condition. Every injured part contains sensory nerves. These sensory nerves communicate with a spinal centre. That spinal centre communicates with the vaso-motor nerves controlling the bloodvessels of that part. Their injury damages the sensory nerves, and is provocative of inflammation in the wound. It has been long known that the application of sedatives to the part, as

opium and laudanum, and, more recently, that great obtunder of peripheral nervous sensibility, carbolic acid, relieves inflammation. In my opinion, the influence of carbolic acid is due to the fact that it obtunds sensibility, and thus lessens vaso-motor action.

Now it has been known by the experiments made in the Johns Hopkins laboratory, that quinine has the power of destroying spinal reflex excitability, and it is upon this principle that I administer cinchona in some of its forms during the healing of wounds. In all operations and wounds, and in the introduction of the catheter or bougie, I always cinchonize my patient to a certain extent. I know that I am speaking in a region where the influence of malaria may not be recognized. My practice is in a malarial country on the banks of the Savannah River, but if I was sure that there was no malaria I should still give quinine.

Dr. Briggs. There is no occasion for any reply, for the paper has not been discussed. There are no points to answer, and I have no remarks to make.

A RÉSUMÉ OF THE ETIOLOGY, PATHOLOGY, DIAGNOSIS, AND TREATMENT OF MORBUS COXARIUS.

BY JOSEPH C. HUTCHISON, M.D., LL.D.,
OF BROOKLYN, NEW YORK.

[Read June 2, 1882.]

My object in presenting to this Association the following conspectus of morbus coxarius, is to elicit candid and independent discussion of the subject, with the hope that we may leave it better understood than we found it.

1. *Etiology.*—Hip-joint disease usually occurs in children who are the victims of a morbid diathesis, commonly known as tubercular or scrofulous, and may be developed by very trifling injuries of the joint. In many cases it originates spontaneously, the most rigid investigation failing to trace its cause to anything like an injury. Sometimes the constitutional dyscrasia, instead of being a cause, is developed as a consequence of the local disease.

2. *Pathology.*—This disease, like other joint diseases, may begin either in the bone or in the synovial membrane. In a majority of cases it originates in the bones which enter into the formation of the joint—the femoral head and the acetabulum—but sooner or later all the tissues of the joint become involved.

3. *Diagnosis.*—An early diagnosis is important. It will often enable us, by proper treatment, not only to prevent the disease from passing into the destructive stage, but also to secure perfect recovery without deformity and with complete mobility.

The most characteristic symptom, irrespective of the tissue in which the disease began, is rigidity of the muscles which pass from the pelvis to the femur, and fix the thigh upon the pelvis

more or less completely. This symptom for diagnostic purposes is of more value than all others. In the very beginning of the disease the thigh will often move to a limited extent in the socket without pain; when, however, it is fully flexed on the abdomen, or fully extended, the pelvis moves with the femur, and as the disease advances the rigidity increases. If, in addition to this symptom, inward rotation of the thigh and abduction are limited, all doubts are removed. These symptoms are pathognomonic of hip disease. All other early symptoms are common to other morbid conditions. In the more advanced stages of the disease there is seldom any difficulty in recognizing it.

4. *Pain from Reflex Muscular Spasm.*—The most important influence by far in the production of pain in hip disease is reflex spasm of the peri-articular muscles. Muscular spasm causes pain, (1) by pressing together the inflamed and sensitive joint surfaces; or, (2) if the joint surfaces are intact, by the pressure communicated through them to the inflamed cancellated tissue; or (3) by rubbing and grinding the inflamed and granulated synovial membrane when the disease is limited to that structure. Muscular rigidity must not be confounded with reflex spasm. The former may exist to such a degree as to produce apparent anchylosis, and yet the patient may experience no pain, but where reflex spasm takes place, violent pain is developed. Clinical observations teach us that whatever increases pain, as a rule, increases the articular disease, and that recovery takes place *pari passu* with the diminution of pain.

5. *Treatment.—Rest* to the joint is of the first importance in the treatment of hip disease. Its value cannot be overestimated. The joint is put at rest by nature's methods more efficiently than can be done by any artificial appliance. Fixation of the joint is secured by a persistent rigidity of the peri-articular muscles; this prevents the injurious effects of friction on the inflamed surfaces. It is doubtful whether absolute immobility of the joint can be obtained in any way, and experience shows that it is not necessary. Extension also contributes to fix the joint, but its most important effect is to resist contraction of the muscles, with

its bad consequences, to which we have referred. It also corrects malposition of the limb. The ease and comfort of the patient are likewise promoted by removing the pressure of the weight of the body from the diseased joint.

By these expedients we relieve pain and prevent or overcome deformity, and by doing this we put the patient in the best possible condition for recovery. We do not cure the disease, but we remove the conditions which maintain and aggravate the morbid irritation which, if unchecked, prevents any real improvement.

All the indications for the treatment of hip-disease are fulfilled by the simplest expedients, viz., by the use of a pair of crutches

Fig. 1.

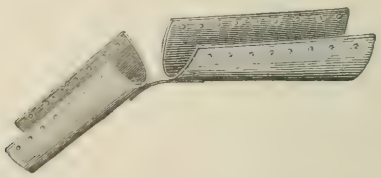
Fig. 2.

and a high shoe on the well side (Figs. 1 and 2). Extension, and the prevention and removal of deformity, are obtained by the weight of the suspended limb, which is equal to one-fifth of the

weight of the entire body, and is greater than can be borne, when the child is treated in bed with weight and pulley. Immobility, sufficient to prevent injurious motion and friction, is secured chiefly by rigidity of the peri-articular muscles, aided by intra-capsular effusion and the voluntary efforts of the patient to keep the joint at rest. No apparatus is necessary to effect immobility. Nature fixes the joint more efficiently and more gratefully to the patient than the most ingenious appliances that art has suggested. No apparatus, yet devised, is so satisfactory as the crutches for removing the weight of the body from the diseased joint.

Instead of putting a high shoe on the well side, Dr. R. J. Levis has suggested lifting the foot of the diseased side from the ground, by flexing the knee and keeping it flexed by a silicate of sodium or plaster splint, which, after being cut open in front, is removable and readily reapplied by bandaging or by lacing it up. The flexed position is maintained during the day when the patient is walking on crutches, and the splint is removed at night. This substitute for the high shoe on the well side still further simplifies the treatment. I have adopted this suggestion of Dr. Levis in cases treated in hospitals, but instead of using his silicate of sodium or plaster splint, I use, because it is more durable and convenient to apply, an appliance which consists of two splints, connected posteriorly by a strip of steel bent to the required angle (Fig. 3); one splint embracing the lower two-

Fig. 3.



thirds of the thigh, the other, the upper two-thirds of the leg. These are most conveniently made of shellac cloth, in use at the Brooklyn Orthopædic Infirmary. The splints having been cut the required size, are made flexible by dry heat in an oven and quickly applied to the limb over a long closely-fitting stocking

leg or bandage, and moulded to the inequalities of the leg and thigh by a tight bandage and the hands of an assistant. When cool they are removed, the edges trimmed, and holes punched in each border about half an inch from the edge, and one inch apart for lacings. They are now ready to be attached to the strip of steel which has been previously bent to an angle sufficient to raise the foot far enough to prevent the toes touching the floor. The attachment is made by passing two rivets through each splint and corresponding holes in the steel. The strength of the steel must vary to meet the requirements of each case. For a child between six and ten years of age, a strip one-half inch wide and one-eighth of an inch thick, will be found sufficiently strong. To secure greater comfort the splints may be lightly padded and lined with chamois; although this is unnecessary when applied over the leg of a stocking or a bandage. In summer the splints can be perforated if desired. It is removed at night, and a weight and extension apparatus may be applied if necessary.

When the thigh is flexed upon the pelvis by tonic contraction of the psoas muscle, so that the toes do not touch the floor when standing on crutches, neither the splint nor the high shoes are necessary, until the flexion has been overcome by the weight of the limb. Walking or standing upon the crutches three or four hours daily appears to relax the muscular rigidity sufficiently to prevent reflex spasm, and consequently pain at night. If it does not, the weight and pulley may be used at night, but I have not found this necessary.

The plan of treatment herein described, I advise in all forms and all stages of the disease, whether first, second, or third, until a cure is completed, except for patients who are too young or too feeble to use crutches, and in rare cases, where acute disease is suddenly developed, attended with great constitutional disturbance and excruciating pain, increased by the slightest movement of the limb or shaking of the bed. Such cases must be treated in bed with weight and pulley, until the acute symptoms have subsided.

My connection with an Orthopædic Infirmary, and with four

general Hospitals, as visiting or consulting surgeon, has afforded me ample opportunities for testing the various appliances that have been devised for the treatment of hip disease, but for the last five years I have used the plan above described with greater satisfaction than any other. It is simpler, more efficient, more comfortable to the patient, and less troublesome to the surgeon, and I unhesitatingly, but deferentially, commend it to the attention of the profession.

DISCUSSION.

Dr. Basil Norris, United States Army.

I should like to know whether Dr. Hutchison recommends a splint moulded to the body and attached to the thigh.

Dr. Hutchison. The apparatus with a splint moulded to the body, and extending from the shoulder-blade downward, is the apparatus of Mr. Owen Thomas, of Liverpool.

Dr. H. F. Campbell, of Augusta, Georgia.

I have only a remark to make about a case which came under my care in the irritative stage. The patient was a young boy serving as a clerk in a store. He could not well afford to stop working. I applied an elevated shoe on the well side, and advised him to use crutches in the daytime. At night extension was applied to the affected limb by a weight and pulley. Marked improvement occurred in three or four months. I advised him to continue this plan of treatment until every evidence of irritation had disappeared.

Dr. DeF. Willard, of Philadelphia, Pennsylvania.

The most important point in disease of the hip-joint is early diagnosis. It is from this that we may hope to obtain the best results. The majority of cases that have gone on until any one can recognize them, are beyond the best cure, *i. e.*, cure with mobility.

This question of muscular immobility is one of great importance. It is a peculiar, pathognomonic sign. It is one on which we can rely with almost absolute certainty. When it is present, we

should always treat the patient as if we were sure that hip-joint disease existed. It is then in the incipient stage. It may be only ten days after the injury. I have seen it within this time. If treatment of the case is commenced at this time, it can be cured by absolute rest, and prevented from going on to the second and third stages. This is especially true in traumatic cases. The presence of muscular rigidity should at once lead us to institute treatment for hip-joint disease. I believe that this is the most reliable of all the symptoms.

In regard to the treatment, I have practised the plan of Dr. Hutchison for a number of years, but I do not think it is wise to pull on the peri-articular muscles. I think that this increases their irritability, and thus gives another cause of inflammation; while, if we simply have some material which will adapt itself to the part and make artificial support, we will give comfort and ease to the patient. This will fasten the joint, and taking the strain from these muscles, will give comparative rest. I do not think that anything will give complete rest. I believe that no apparatus can sustain the weight of the body and allow the patient to walk.

Dr. William A. Byrd, of Quincy, Illinois.

I have been much interested in the exposition of Dr. Hutchison's method. Since becoming acquainted with Mr. Owen Thomas's method of treating hip-joint disease, I have used his splint with good results.

Dr. R. A. Kinloch, of Charleston, South Carolina.

I rise to say only a few words. I have used Dr. Hutchison's splint with good success in a few cases. It does not strike me, theoretically, that the flexed position is an improvement upon the straight position. I am inclined to object to it, as Dr. Hutchison himself appears to object to it when at a right angle, inasmuch as I think that the limb is then more exposed to sudden rotation from the leg acting as a lever when the patient is moving about. We know that sudden rotation is very prejudicial. I think the danger is apt to be increased by this position of the limb.

Dr. R. J. Levis, of Philadelphia, Pennsylvania.

Dr. Hutchison has given me credit for flexing the limb on the diseased side, and he states that I flex it at a right angle. That, I have not insisted upon. In the case that Dr. Hutchison saw, the

limb was flexed at a right angle. All that is necessary is to flex the limb sufficiently to keep the foot from the floor. I claim no originality for this, as it is dependent upon the principles laid down by Dr. Hutchison. I believe that by simply flexing the leg on the diseased side, we can get along without elevating the limb on the sound side.

Dr. Hutchison. I have only a few words to add. First, in regard to compelling the muscles to hold the limb still. I have not found it necessary to apply a splint for this purpose. The muscles will hold the limb still. It is no effort for them to do so.

I have not found the use of Owen Thomas's splint necessary. I have never used it. Among other reasons, I have not used it because it must be extremely inconvenient to the patient, because it fixes the patient in the erect position. He cannot sit down. If he wants to change his position, he must lie down. I believe that this apparatus fixes the joint more thoroughly than any other splint. The splints that are ordinarily used for this purpose do not fix the joint. The Davis and the Sayre splints do not fix the joint. It is unreasonable to think that this can be done with a splint extending but a few inches above the joint. In order to fix the joint it is necessary to carry the splint some distance up the side of the trunk. It is necessary to fix the pelvis. In such splints as the Davis the motion occurs at the lumbo-sacral junction and at the lumbar vertebræ.

I am glad to make the correction Dr. Levis has suggested. I have not found it necessary to make extension at night. There are cases in which it is necessary. If you keep a patient walking about or standing for several hours the muscles become sufficiently stretched to require no stretching at night. There is, however, no objection to it.

A CASE OF BONY UNION AFTER IMPACTED INTRA-CAPSULAR FRACTURE OF THE NECK OF THE FEMUR.

By N. SENN, M.D.,
OF MILWAUKEE, WISCONSIN.

[Read June 1, 1882.]

I DESIRE to exhibit to the Fellows of the Association a specimen showing bony union after impacted intra-capsular fracture of the neck of the femur, and to read the following history of the case:—

Mrs. R., aged 75 years, was admitted into the Milwaukee Hospital, March 15, 1880. With the exception of chronic bronchitis and pulmonary emphysema, she never suffered from any serious illness. A few days before admission she slipped and fell on the sidewalk, the weight of the body falling directly on the left hip. She suffered great pain immediately after the injury, and was unable to move the limb. A slight contusion over the left trochanter major indicated the point where the injury had been received; the swelling about the hip was only slight. The left foot was everted, and on accurate measurement, the limb was found shortened one-half of an inch. An attempt to flex the thigh upon the pelvis caused intense pain; on rotating the limb, the arc of circle described by the great trochanter was found somewhat diminished. During the examination, which was conducted very carefully, no crepitation could be felt at any time, nor was I able to overcome the shortening by gentle traction. The patient suffered considerable pain in the hip-joint and in the region of the trochanter minor. The age of the patient, the manner in which the injury was received, and the result of the examination induced me to diagnosticate an impacted intra-capsular fracture of the neck of the femur. As there was no tendency to displacement, the patient was simply placed in the recumbent position on an even mattress, and the limb supported on each

side with sand-bags. The patient was kept in this position for nearly three months; at the expiration of this time she was allowed to walk on crutches. During the whole treatment the limb was measured occasionally, and it was found that after the third week the shortening gradually increased until it reached an inch and a half, when the treatment was suspended. The patient recovered very slowly, but, at the expiration of a year, she was able to walk well without crutches, using only a cane.

She was re-admitted into the hospital February 20, 1882, suffering from catarrhal pneumonia, from the effects of which she died two days subsequently. After death the limbs were measured very carefully, and the shortening amounted to an inch and a half, the left foot strongly everted, motion in the hip-joint limited. The capsule of the joint, especially the upper portion, was thickened and firm, and bridges of fibrous bands connected the line of fracture with the anterior portion of the ligament. On the anterior surface of the neck the direction of the fracture could be clearly traced from below upward, and from within outward, but not extending beyond the insertion of the capsular ligament. The line of fracture is elevated, and presents a serrated appearance. Posteriorly the head of the bone was in close proximity to the posterior inter-trochanteric ridge. A slight depression on the articular cartilage marked the point of contact with the inner surface of the capsular ligament. Impaction had evidently taken place at the expense of the posterior compact portion of the neck.

A portion of Adams's arch, which had been implanted into the lower fragment, could be distinctly seen in the spongiosa, on making a vertical section. A vertical section through the neck, head, and trochanter, revealed a white line of very compact bone traversing the cancellous tissue of the neck near the shaft in an oblique direction, corresponding to the line of fracture on the anterior surface of the neck.

The anterior half of the specimen has been submitted to the boiling test without affecting the union of the fragments, hence there can be no doubt as to the union by bone.

The bone outside of the capsular ligament presents no sign of callus or any other evidences of injury or disease.

The specimen is herewith submitted, in order that it may be examined by the Fellows of the Association.

DISCUSSION.

Dr. H. F. Campbell, of Augusta, Georgia.

Dr. Seldon, of Norfolk, Virginia, reported some remarkable cases of fracture in old people, in which bony union was found at the post-mortem examination. They were stated to be instances of intra-capsular fractures, but I could not say whether they were impacted or not.

Dr. Moses Gunn, of Chicago, Illinois.

I desire to make but one or two remarks on this specimen. Bony union of entirely intra-capsular fractures is so rare that it will always arrest attention, and where we have any such claim made, it behooves us to scrutinize the specimen very closely. Dr. Senn regarded the fracture as entirely within the capsule, and preserved the ligaments in order that that fact might be seen.

I have just examined the specimen for the first time. I find a fracture through the neck. I see, however, no evidence of impaction. I see the line of dense bony union. On examining the upper part of the section on the posterior fragment, I trace distinctly the capsule and a positive unmistakable connection with the root of the neck on the outer fragment; so that the line of fracture falls at that point on the outside of the ligament, and it is on this account that bony union has occurred. The outer fragment had other support than that which reached it through the round ligament. The evidence of the specimen is that the outer fragment had a vascular supply from the exterior.

Dr. S. D. Gross, of Philadelphia, Pennsylvania.

I would ask Dr. Gunn whether he has ever seen a case of intra-capsular fracture of the neck of the thigh-bone in which bony union had occurred. I have never in my experience met with an instance of the kind.

Dr. Moses Gunn. I never have, and I further say that I never expect to see such a result.

In reply to a question by Dr. Gross, Dr. W. T. Briggs, of Nash-

ville, Tennessee, said: I have examined probably eight or ten specimens of intra-capsular fracture, with special reference to the existence of bony union, but I have never seen it, and do not expect to see it.

CONDITION OF THE CICATRICES AFTER COMPLETE EXTIRPATION OF THE BREAST FOR CARCINOMA.

By SAMUEL W. GROSS, M.D.,
PROFESSOR OF THE PRINCIPLES OF SURGERY AND CLINICAL SURGERY IN THE JEFFERSON MEDICAL COLLEGE OF PHILADELPHIA.

[Read June 2, 1882.]

I desire to exhibit four patients from whom I have removed the mammary gland on account of carcinoma. My object in bringing the cases before the Association is to demonstrate the condition of the cicatrices at various periods after the operation; as it had been theoretically urged at a former meeting that the thorough operations practised by me might be followed by morbid conditions of the cicatricial tissue.

Case I.—In the first patient the cicatrice is soft, mobile, of a natural tint, and hardly an inch in width. The entire breast, with its coverings, the pectoral fascia, and a mass of axillary glands were removed on the 4th of September, 1878. Forty-five months have elapsed since the operation, and there is not the slightest evidence of recurrence. At the date of the operation the woman was forty-eight years of age.

Case II.—On the 6th of August, 1879, or thirty-four months ago, I extirpated the entire breast of a married woman, thirty-nine years of age. The axillary glands, on exploration, were found to be free from involvement. The huge wound had entirely closed in thirteen weeks. The cicatrice, which is at its widest part nearly three inches broad, is soft, mobile, and free from discoloration. The disease has not recurred.

Case III.—I removed the entire breast and ten axillary glands from a married woman, forty years of age, on the 28th of December,

1881. The breast was small, and the tumor ran from above downwards and inwards toward the nipple. The edges of the wound were united, but subsequently reopened; and the cicatrice, which is still red, is only half an inch wide. There has been no recurrence of the disease.

Case IV.—I extirpated the entire breast of a single lady, forty-five years old, on the 1st of March, 1882. The axillary glands were not affected. The cicatrice is scarcely an inch wide; but, in consequence of too early and too frequent use of the corresponding arm, it became the seat of several superficial excoriations. The patient is free from recurrence.

THE INTRA-PERITONEAL METHOD OF TREATING THE PEDICLE IN OVARIOTOMY.

With a Report of some Experiments made.

By J. EWING MEARS, M.D.,
GYNÆCOLOGIST TO JEFFERSON MEDICAL COLLEGE HOSPITAL, SURGEON TO ST. MARY'S HOSPITAL, ETC.

[Read June 2, 1882.]

I wish to call the attention of the Fellows of the Association to some experiments which I have made, in connection with Dr. Morris Longstreth, on the intra-peritoneal method of treating the pedicle by the ligature after ovariotomy, and to make a few remarks upon the various methods of treating the pedicle.

As is well known, the first operation for the removal of ovarian cyst was performed by Dr. Ephraim McDowell, of Kentucky, in 1809. In this operation the pedicle was secured by a ligature, one or both ends of which were drawn through the incision in the abdominal wall and placed outside. In this way the pedicle was drawn up and placed in contact with the abdominal incision.

In 1821, Nathan Smith, of New Haven, Connecticut, according to the statement of Peaslee and others, ignorant that the operation of ovariotomy had been performed, ligated the pedicle in this way, *i. e.*, he ligated all the vessels of the pedicle separately, using for that purpose animal ligatures in the form of strips of kid-glove. He ligated the vessels, cut the ends of the ligatures close to the knots, and returned the pedicle to the abdominal cavity. This is the first instance of ligation of the vessels of the pedicle, and also the first instance in which the ligature was cut close to the knot and the pedicle returned to the abdominal cavity with the ligatures in position.

Torsion was afterwards performed—torsion of the vessels separately, torsion of the pedicle *en masse*.

Later, in 1860, Mr. Simpson, of Edinburgh, suggested the use of the acupressure needle for the purpose of constricting the pedicle. In 1858, John L. Atlee, of Lancaster, Pennsylvania, used the écraseur, in that way, dividing the pedicle and crushing the vessels. Baker Brown, of London, in 1864, used the actual cautery; divided the pedicle with the actual cautery, and thus seared the stump of the pedicle. Spiegelberg, of Breslau, used the galvano-cautery in 1868. Macleod, of Glasgow, twisted off the pedicle by means of two pairs of strong forceps, and in that way controlled the hemorrhage. In 1869, Dr. Miner, of Buffalo, New York, enucleated the pedicle. In 1869, Peaslee, of New York, introduced a method which consisted in the introduction of a tube into the abdominal cavity, around which the pedicle was secured by double ligature. At the expiration of two or three days these ligatures were divided by a peculiarly shaped knife passed into the tube, and the ligatures, with the tube, were then withdrawn from the cavity.

These are in brief the different plans which have been adopted since 1809 in the treatment of the pedicle after ovariotomy in the intra-peritoneal method. All these forms can be divided into two classes: 1, those which include the application of ligatures to the pedicle; 2, those which dispense with ligatures. The first includes the method of McDowell, Nathan Smith, and of the other operators, in which the ligature is applied either to the vessels separately, or to the pedicle in two or more portions, or *en masse*. The methods which dispense with the ligature are torsion, the acupressure needle, the écraseur, the cautery in both forms, and the method of Dr. Miner, of Buffalo, *i. e.*, enucleation. It will be seen from this that the first method adopted was the intra-peritoneal.

I wish to mention another intra-peritoneal method, termed the sub-peritoneal method, in which the pedicle is constricted beneath the layer of peritoneum. It was suggested by Dr. Hayes, of Dublin, and consists in entering the needle beneath

the peritoneum, carrying it around the pedicle, and bringing it out at the point of entrance.

The intra-peritoneal method was practised until Mr. Hutchinson, of London, introduced the clamp. Then the pedicle was treated outside of the abdominal cavity, being brought between the lips of the abdominal wound and clamped. There are some modifications of this method, but not many. They consist in securing the pedicle between the lips of the wound by acupressure needles, hare-lip pins, or some other method.

Spiegelberg and Waldeyer were the first to investigate the changes which take place in the pedicle after its ligation and return to the abdominal cavity. These experiments were made in 1868, and they were corroborated by Maslowsky, of Russia; he going further in his investigations, endeavored to show what changes occurred in the pedicle after the application of the actual cautery. So far as I know, no experiments which show the changes occurring in the pedicle after ovariotomy, have been made in this country. Therefore, as a matter of interest, I exhibit to the Fellows of the Association the results of some of the experiments, which were performed in connection with Dr. Morris Longstreth.

It is proposed to continue this series of experiments on the lower animals, and to present further results at a future meeting of the Association.

Rabbits were selected as the animals upon which the experiments were to be made, and ligatures of various kinds were used.

In the first experiment both ovaries were removed. On one side a simple catgut ligature of the ordinary size employed for the ligation of vessels after operations was used. On the other side a chromatized catgut ligature was used, *i. e.*, a ligature which had been submitted to the action of chromic acid (one part of acid to five of water) for three weeks. This mode of preparation has been instituted for the purpose of hardening the ligature so that it will last longer; the fear being that the ordinary catgut ligature would soon disintegrate, and that hemorrhage might thus occur after its application. This ligature was prepared in St. Mary's Hospital, as are all of the anti-

septic dressings which are there used. It was of large size, and had been prepared for the purpose of uniting the fragments of bone in an ununited fracture, in which case it was proposed to apply the ligature and allow it to remain *in situ*. At the end of two weeks the rabbit was killed, and a post-mortem examination was made. On opening the abdominal cavity there were no evidences of inflammatory action in the general peritoneal cavity. It was found that the ligatures (both the simple catgut and the chromatized catgut ligature) were completely encapsuled.

These specimens will show distinctly the conditions which existed. I also exhibit a drawing of a microscopical section showing this to better advantage than it can be seen in the specimen. In applying the ligatures very little force was used. Only sufficient force was used to control the hemorrhage. This is a point to which I wish to direct particular attention as an important point in the discussion of this subject. The ligature was cast around the pedicle *en masse* with sufficient force to control the hemorrhage, and the ovary was removed. In applying the ligature to the pedicle it was embedded in the tissues, and at the point of application the two portions of the pedicle came in contact and became adherent. A layer of lymph covered the ligature—even the knots were covered. The layer of lymph passed over the divided ends of the ligature. Thus a bridge was projected between the proximal and distal portions of the pedicle. What had occurred in the distal—in the free portion—the raw surface? We found that it was also covered with a layer of lymph, and on one side it had become adherent to the adjacent portion of the broad ligament. On the other side the stump was free and covered with a layer of lymph.

An interesting point in connection with this is to ascertain, especially on the side on which the stump was free, how the stump was nourished. I think there is no doubt that it was nourished in two ways. In the first place, by this bridge conducting nutrition from the proximal to the distal portion; and in the second place by insterstitial nutrition, *i. e.*, by nutrition passing through the tissues beneath the ligature, which was sufficient to nourish the distal portion. On the side where the stump had

become adherent to the broad ligament, no doubt the distal portion formed vascular connections, and in this way was nourished.

The specimens taken from the first rabbit, which I herewith present, make it very evident that the ligature is encapsulated.

Here are the specimens removed from another rabbit, in which a silk ligature and an ordinary linen thread were used. These ligatures had been soaked in a solution of carbolic acid (one part of acid to twenty parts of water). This rabbit was killed at the end of four weeks. On examination of the specimens it was found that the ligatures had not disappeared, although not completely intact. Between the meshes of the ligature, cells had wandered and the ligature was in a process of disintegration. This was also seen to a certain extent, in the catgut ligature in the first experiment. This indicates the manner in which the ligature disappears—by disintegration and by the development of cells which get into the meshes; and in this way absorption takes place.

In these specimens, at the end of four weeks, a layer of lymph, not so voluminous, was found, showing that there have been some absorption, shrinking, and development; and after a time only a thin, fully developed layer of lymph covering the ligature and also the distal portion of the pedicle would exist.

Mr. Spencer Wells has objected to the employment of the intra-peritoneal method of treating the pedicle. He feared that the distal portion of the pedicle would slough off, and would be thrown into the abdominal cavity, and thus produce inflammatory action leading to a fatal termination. He has, however, been convinced to a certain extent by experiments of Spiegelberg and Waldeyer, although he still adheres in great measure, I believe, to the extra-peritoneal method of treating the pedicle. He had in one of his cases a fatal termination after the employment of the intra-peritoneal method, in which he found that the distal portion had attached itself to a coil of intestine; this portion was found in a sloughing condition and lying in a pool of pus.

In the second rabbit it was found that adhesions had taken place between two portions of the intestine; adhesions between the abdominal wall and the intestine. Adhesions of the intes-

tine to the abdominal wall in the line of the incision are just as liable to occur as adhesion of the pedicle in the extra-peritoneal method. What might occur also if the pedicle became adherent to the intestine is this: The stump might attach itself between two coils of intestine, and, suppuration occurring, the pus would accumulate as in a pocket and escape possibly through the intestine.

The conclusions which may be deduced from one or two experiments may be of little value; still they may be taken for what they are worth. I think that they show that the intra-peritoneal method is in every way a safe method; that nature takes care of the ligature, and that it takes care of the stump of the pedicle. A point of importance to which I think that attention ought to be called, is in regard to the amount of force which is used in the constriction of the pedicle. It is desirable to simply control the passage of blood through the vessels. It is very desirable to permit the so-called interstitial circulation, nutrition being maintained beneath the constricting ligature through the connective-tissue interspaces.

Subjoined is the report of Dr. Morris Longstreth upon the appearances presented post mortem:—

Rabbit No. 1.—Right ovarian pedicle tied with carbolized catgut; left side tied with chromatized-carbolized catgut—large sized.

Operation, April 10. Autopsy, April 25th, two weeks after operation.

The animal was killed with chloroform, and the body immediately opened. The specimens removed for microscopic examination were placed in Müller's fluid.

At the autopsy, no evidence of inflammation of the peritoneum was found. All the organs of the body were healthy.

At the seat of the operation, the extremities of the uterine horns were found attached to the adipose tissue embedded in the lateral or broad ligaments, by which the uterine extremities, their fimbriated terminations, and the ovaries are held in place. This adipose tissue formed a cap over the free end of the pedicle made by the removal of the ovary. The adhesions were firm

and well organized, but the adjacent parts showed no evidences of inflammatory changes or lymph.

The bloodvessels behind the seat of the ligature were enlarged and very full of blood, and in the new tissue, over and at the point of operation, were seen fine red lines resembling newly formed capillaries.

The ligatures on both sides were thoroughly covered by either the old or the newly formed tissues. The catgut was entirely concealed, except on the left side, where the heavier strands had been employed; here the ends of heavy, blackened strands could be seen and felt protruding from the crease or furrow behind the tip of the pedicle, formed by the circle of the knot. These projecting ends of catgut were, however, completely covered, although visible through the layer of semi-transparent lymph, and thus they presented a smooth surface to the surrounding peritoneal membrane in contact with them.

The process by which the ligatures were covered, as seen by the naked eye, and in the sections under the microscope, was as follows: The compression of the pedicle, at the moment of tying the ligatures, caused a deep sinking-in of the tissues at this line, and on cutting away the ovary and its attachments, the parts on both sides of the ligatures well up over its strands, nearly covering it, but not completely. The opposed surfaces of the peritoneum, thus brought in contact over the ligature, adhered, and their union was cemented by a thin layer of lymph. By this process, the parts *beyond* the point of ligation were placed, probably very rapidly, in organic or vital relations with the parts *behind* the ligature.

The free extremity of the pedicle and all portions beyond the pedicle, though deprived immediately of blood circulating in the normal channels, received a measure of nutrient material, sufficient to maintain its vitality. This material could find its way into the tip of the pedicle through connective-tissue interspaces in the part compressed by the ligature. This compression was sufficient to prevent the passage of blood in the vessels, and thus control the hemorrhage, but was not firm enough to stop the nutrient fluid derived from the blood from penetrating. The

nutrition was maintained, and rapidly increased by the route of the peritoneal surface newly adherent over and around the ligature.

The almost immediate effect of the partial strangulation on the tip of the pedicle is, that the tissues of this portion become infiltrated with small cell-elements, derived partly from the cells of the part, partly from an immigration of white blood-cells. This portion, hence, becomes filled with material capable of covering the free extremity with a granulation tissue or a plastic lymph. If this material is well nourished, its capacity for rapid organization is very great, and the newly capped raw extremity may heal almost immediately. In this rabbit, however, the free extremity coated with lymph came in contact with the adjacent peritoneum, and adhered to it; between these two surfaces the microscopic sections show that union has taken place.

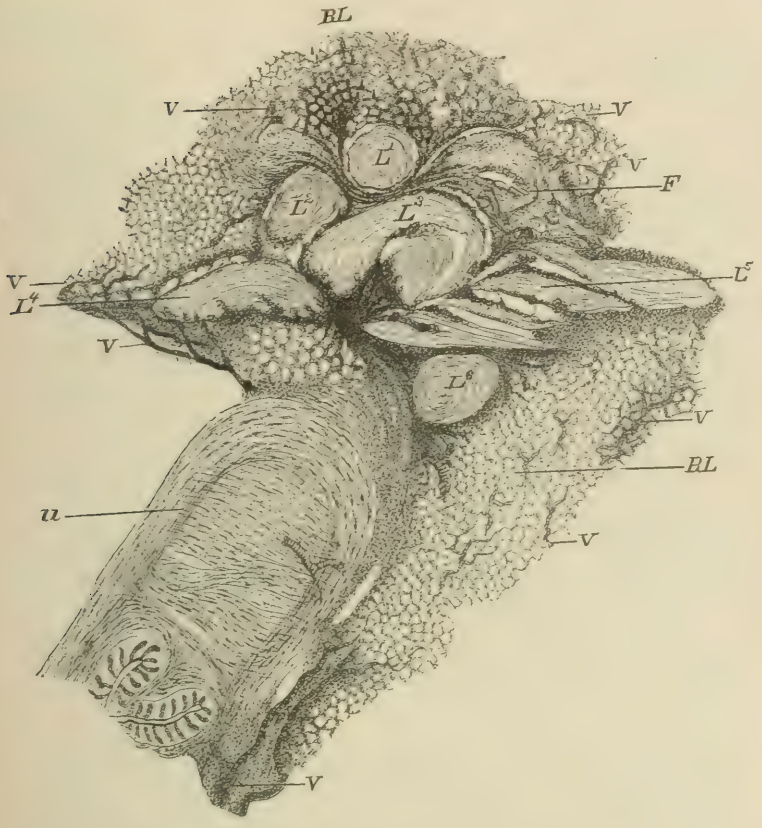
The ligature at the end of two weeks is well preserved, the knot well tied, and the whole firmly inclosed by a covering of organized or organizing materials. In portions there is presented the appearance of a vascular protrusion between the strands of the ligature. And if this foreign body is ever removed, the object is probably effected after the vessels have been well developed around and through its strands.

Rabbit No. 2.—Right ovarian pedicle tied with silk; left ovarian pedicle tied with linen ligature.

Operation, May 1st. Autopsy, May 29th, four weeks after operation.

The animal was killed in the same manner as No. 1. The autopsy showed no form of disease, except several hydatid cysts attached to the omentum just below the transverse colon. The microscope showed the presence of living echinococci.

The uterus in this animal was probably nulliparous, and its tissue as well as the surrounding parts were less voluminous. No adhesions were found in the abdominal cavity around the seat of the operations. Some peritoneal adhesions of the intestines existed, which were very possibly due to the introduction of the hypodermic needle in the administration of the solution of curare at the time the abdomen was opened. The points of the two pedicles presented about similar conditions.



U. Uterus. *V.* Vessels. *F.* Fallopian tube or uterine cornu. $L^{1, 2, 3, 4, 5, 6}$. Ligature. *B L.* Broad ligament.

The broad ligament arises from the uterus, and passes up between $L^{4, 5}$, and ^{6}, and under L^{3}. The parts of it to the left (in contact with the uterus) are loaded with cells and inflammatory material. To the right is seen its fatty structure, somewhat inflamed, and containing small vessels, *V*. It has been caught also by L^{6}, adheres to L^{4} and ^{5} by lymph, cells, etc. The upper portion of the broad ligament has been caught between $L^{1, 2}$, and ^{3}. Its cut extremity has fallen over to the left, over $L^{1, 2}$, and ^{4}, to which it is adherent by inflammatory material, just to the left of L^{4}. The upper and lower portions of the ligament here come into contact and adhere. At this point, the vessels *V* probably anastomose. The stump of the Fallopian tube, or uterine cornu, *F*, has been caught between L^{1} and L^{3}. Its cut portion has fallen down and to the right, the lower portion rests upon, and is adherent to, L^{5}.

Section made from left ovarian pedicle in rabbit No. 1. Ligature, carbolized catgut.

The healing process had taken place without the intervention of adhesions to the surface of the broad ligament. The end of the pedicle was covered by a layer of organized material, and

was much less in bulk than the same parts in the former animal. The ligature of silk, with which the right ovarian pedicle was tied, remained in position.

DISCUSSION.

Dr. WILLIAM A. BYRD, of Quincy, Illinois.

One year ago I performed ovariotomy, and employed the intra-peritoneal method of securing the pedicle. The patient died, and at the autopsy I found the ligature encapsulated, as is observed in the specimens which have been exhibited by Dr. Mears.

I see no reason, however, why, in abdominal surgery, we should not resort to torsion. I do not see any reason for putting any foreign substance into the abdominal cavity. This is carrying Dr. Miner's method a little further. Since I have seen Mr. Lucas and Mr. Bryant use torsion upon the femoral artery after amputation of the thigh, without having secondary hemorrhage, I think there would be no danger in employing the same method on the vessels of the pedicle after ovariotomy.

Dr. MOSES GUNN, of Chicago, Illinois.

This subject is exceedingly important. It is not a question of what is the best means of controlling hemorrhage, but of what becomes of the ligature which is thrown around the pedicle after ovariotomy and returned to the abdomen. That animal ligatures will be absorbed if they are so prepared as to resist decomposition, is a foregone conclusion theoretically, and proven by repeated experiment. The point of interest and importance is in regard to the vegetable fibre—the linen thread. I believe that only one experiment has been made, and in that, the examination was made at a comparatively early period, but it shows that the thread was being infiltrated by wandering cells, and that it was being disintegrated and destroyed. The experiment should, I think, be carried to its end, to see if we can determine what becomes of the vegetable fibre. If it is proven that the vegetable fibre will be absorbed and carried away, that will be something decidedly new and important.

Dr. J. Ewing Mears, of Philadelphia, Pennsylvania.

I had the opportunity, in a recent ovarian operation, in which I proposed to use the intra-peritoneal method, applying a carbolized catgut ligature to the pedicle, to watch some of the changes which took place in the ligature. It was a case in which the tumor was small. The pedicle was very broad and short. I divided it into three portions and tied it with the catgut ligature, but before I closed the abdominal cavity, I found that the pedicle was so short that the ligature had slipped. I then clamped the distal portion of the pedicle, leaving, as I thought, the ligature entirely within the abdominal cavity. During the separation of the clamp one extremity of the ligature protruded through the wound, and I had an opportunity of observing some of the changes which occurred in it below the clamp. I pulled out this ligature day after day; and, finally, at the expiration of two weeks, I found nothing but a gelatinous soft mass.

Now it seems to me that, as Dr. Gunn has said, the process which takes place in the disappearance of the ligature—the process which we have observed in the experiments—is by separation of the meshes of the ligature; that is, introduction of cells between the meshes of the ligature, breaking up the fibre, and in that way gradually accomplishing its disappearance. This takes place in the animal ligature, and I believe also in the vegetable fibre. The silk ligature is not purely an animal ligature. In the ordinary silk ligature there is an admixture of cotton to increase its strength.

Dr. Byrd has alluded to the changes which take place after torsion of the vessels; a method which has been employed in the intra-peritoneal method of treating the pedicle. In some forms of torsion of vessels, the portion constricted is not entirely destroyed. I believe that the distal portion does not separate from the vessel, but that inflammatory lymph is deposited, and we have the part which has been crushed revitalized.

HAVE WE ANY THERAPEUTIC MEANS, AS PROVEN BY EXPERIMENT, WHICH DIRECTLY AFFECT THE LOCAL PROCESSES OF INFLAMMATION?

By C. B. NANCREDE, M.D.,
SURGEON TO THE EPISCOPAL HOSPITAL, PHILADELPHIA, PA.

[Read May 31, 1883.]

Believing that the true aim of a paper read before this Association should be merely the indication of the salient points of the subject, in order to elicit as full discussion as possible, I shall omit all such details as are non-essential. It will be impossible for me, in the time assigned, to answer the query forming the title of my paper, except with regard to the early stages of inflammation. Owing to press of work, I have also been unable to pursue my investigations as far as I had wished; and should, therefore, prefer to entitle my paper "A preliminary note as to whether we possess any therapeutic means, as proven by experiment, which directly influence the local processes of inflammation." I trust that any apparent neglect to specifically mention the sources of my knowledge will be set down to the necessary brevity of this paper, and to the fact that all of you must be more or less familiar with the sources from whence my facts are culled.

Careful study of the essential processes of various morbid conditions, has often led to the theoretical employment of therapeutic measures, which, by their apparent practical use, have confidently encouraged their future employment. Clinical proofs, however, always embody the inherent weaknesses of careless or prejudiced observation, with the varying unknown quantity of the vis medicatrix naturæ.

But if in addition we find by experiment that our *à priori*

therapeutics, when experimentally applied in the case of animals, actually produce the precise effects which we have pre-determined will probably prove efficacious, then we can hardly go astray in confidently applying similar treatment to the human subject. If now we find clinical discrepancies, it must be our aim to ascertain the exact limitation of the remedy for good or evil, and the precise indications for its employment, instead of thrusting it contemptuously aside when we fail of the expected result, the failure being probably our faulty method of applying the treatment, and not self-inherent. Upon the other hand, we must always hold before our eyes the experimental mirror of what has been and can be effected, and not expect to do what we have demonstrated is an impossibility.

This paper is avowedly written in advocacy of blood-letting in general, but chiefly of the local abstraction of blood. This once much-vexed question seems again pressing for solution, as the pendulum of unreasoning prejudice and authority, having reached its furthest limit on the side of proscription, is now slowly swinging back to an opposite point, the stability of which must depend upon the possibility of satisfactorily answering the question contained in my title. If the answer be favorable, the revival of blood-letting will be founded upon the only sure basis, viz., that of demonstrated scientific facts which will replace the purely theoretical dicta of authority, which, like the will-o'-the-wisp, led our forefathers into such an erratic use of the lancet, that it wrought not only by its abuse its cure, but an almost total abolition of blood-letting, which I for one regret.

A rapid review of the minute processes of the earlier stages of inflammation, coupled with a consideration of some facts relating to the physiology of nutrition, will form a necessary preface to the body of my paper. I shall refer only to the early stages of inflammation; for, of course, no direct effect can be exerted upon the tissue changes of that process when the inflamed part has returned to its fœtal state—is merely embryonal tissue, only a step further from which is pus.

Examining with a low power, we find that the arteries are normally about one-sixth smaller than the veins, and that "in

every artery a space can be distinguished within the outline of the vessel, which is entirely free from corpuscles." The arterial current is the more rapid, and it is appreciably accelerated at each beat of the heart. As to whether there is, or is not, a primary contraction of the arteries in the first stages of inflammation, seems to depend so much upon the irritant used, that I shall pass the question by as unimportant for our present purposes. The first change noted is an enlargement of the arteries, which become tortuous, the veins following suit; but there is "a time when instead of the arteries being sensibly smaller" than the veins, they "far exceed them in diameter." Note this fact carefully. Contrary to expectation, at the outset the circulation is accelerated in the dilated vessels, but the rate soon becomes normal, and is succeeded by a slowing, then an oscillatory movement, a temporary stagnation, again a resumption of the flow, and finally permanent stasis with crowding of the vessels—the veins especially—with cell elements, so that the previous clear space existing along the walls of the arteries can no longer be detected.

The obstructions seem to consist solely of red-cells, which are so closely packed as to render "their individual form" . . . "scarcely distinguishable." Free diapedesis of the white blood-corpuscles now takes place with exudation of liquor sanguinis, both processes having commenced when incipient stagnation set in. The experiments of Ryneck and H. Weber have demonstrated "that in an injured part the walls of the capillaries become so altered that the liquor sanguinis, instead of transuding from the smaller arteries in quantities just sufficient to balance the absorption, leaks abundantly from the vessels, and that in many cases this is subsequently associated with squeezing out of the leucocytes, or even of the colored corpuscles." The consequence of this free exudation is, that owing to increased pabulum—liquor sanguinis—the cells of the inflamed area multiply until in many instances we have the tissue reverting to the fœtal state, when it consists merely of embryonal tissue, viz., a mass of indifferent cells held together by a small quantity of

intercellular cement, which latter has only to liquefy, and *pus* at once results.

The remaining subsidiary phenomena of inflammation being non-essential are purposely omitted.

To summarize the whole process in the words of Dr. Burdon-Sanderson, "the circulation is at first accelerated and increased, subsequently retarded and diminished;" and "the latter condition is attended with exudation of liquor sanguinis, migration of leucocytes, and stasis."

That the primary acceleration of the blood stream is in some way dependent on reflex nerve action seems clear. At this point our researches into the essential phenomena of inflammation may cease. A few physiological facts demand consideration before I sum up what *à priori* reasoning indicates as to the therapeutics of the early stages of inflammation. Ranvier has shown that the white blood-cell is sluggish, and then ceases to move in the absence of oxygen, and is active in proportion to the amount of that substance present. It is also indisputable that the red cells are the main carriers of oxygen, and that if their numbers are relatively increased to the fluid in a given bulk of blood, especially if, as in inflammation, both the necessity and capability of the cells of the tissue making use of the oxygen are removed, at once an excess of oxygen, *i. e.*, an increased amœboid action of the white cells not only is possible but becomes a necessity. Physiology teaches us that only so much of the constituents of the blood-plasma exude, or are withdrawn by cell-action, as suffice for the normal function of the parts, and that if any excess is present that the lymph-spaces return it into the lymphatics, whence it passes into the general circulation. If, however, the lymph-spaces are compressed by dilated bloodvessels, and crowded with migrated cell elements, the excess of pabulum must be retained, with a consequent stimulus to undue cell proliferation. Precisely this obtains in inflammation. But physiology, likewise, shows us that there is a certain attraction exerted upon the blood current by the tissue cells, according to their varying wants, which not only aids the vis-a-tergo action of the heart, but actually determines,

to a certain extent, the amount of blood present at any given time, for instance, in a gland. If this action obtains in health, there is no reason to doubt it is still more potent in disease. From the physiological fact upon the one side, that only so much pabulum is withdrawn as suffices for healthy function, and, on the other, that in inflammation this material is in great excess, it seems proven, viewed by the light thrown upon the subjects by the experiments of Ryneck and Weber, that in some way the capillary walls are injured by intra-vascular pressure.

To summarize: (1) Intra-vascular pressure injures the vessel walls, aided, perhaps, by the constant passage of the white-cells; in consequence an inordinate amount of blood-plasma exudes which stimulates cell-proliferation. (2) The accumulation and stagnation of the red blood-cells, with the draining off of the liquor sanguinis, causes a relative excess of oxygen, which excites to increased amœboid action the white blood-cells and their consequent migration.

By the study of the phenomena of osmosis, we learn that stagnation of fluid and intra-vascular tension induces outflow, but reverse the latter condition, increase the rapidity of the circulation, and with the constitution of the blood, an inflow must result.

From these studies I am forced to conclude that a theoretically perfect remedy for incipient inflammation must comprehend the following effects: It must either lessen the vis-a-tergo of the heart's action, so as to prevent injury to the vascular walls by over-distension, and the consequent outpouring of liquor sanguinis; it must prevent such an ingress of blood into the affected area as would produce the excess of oxygen, the migration of cells, the blocking up of the lymph-spaces, etc.; or, what would practically amount to the same thing, it must so lessen the difficulty of escape on the venous side, that, howsoever great the vis-a-tergo, a ready draining off, nay, aspiration, as it were, of the venous blood may occur; if possible, both these effects must be produced. The last requisite would be, that the remedy must increase the frequency, while it lessens the force with which the heart acts; for although whatever would lessen the vis-a-tergo would prevent any further outpouring of pabulum, yet, that

which was already exuded must remain, and would have to be consumed before the initiated cell-change could cease. In my experiments on the frog's tongue I noted all the vascular changes described in the earlier part of this paper. When the stasis was complete, a large vein on the distal side was divided, *i. e.*, local bleeding was effected from the vessels directly leading from the inflamed area. I purposely avoided opening the vein on the cardiac side of the phlogosed spot, lest I might, simultaneously, divide the supplying artery. By severing one of the ranine veins, at a point where I could distinctly see that the venous radicles of the inflamed area emptied, I achieved my purpose without further damage.

The effect upon the obstructed vessels was first an oscillation of the blood-disks, then an occasional momentary flow of blood, then suddenly a rapid resumption of the circulation sweeping out the bloodvessels, and apparently restoring them to their normal condition, except at spots where the agents inducing the inflammation had chemically destroyed the vessels or coagulated their contents. I do not presume to say that the white corpuscles betook themselves at once back again into the blood-vessels, for I did not use sufficient amplifying power to observe this, nor was I then dealing with any other processes than the vascular changes. I should have carried my researches much further, had I not seen that nothing was needed beyond a mere corroboration of Gensmer's admirable paper, which I shall now quote.

"At the present time it is generally accepted, that by local bleeding inflammatory stasis is relieved, but this view has not been demonstrated; many adhere to the derivative action of bleeding, and many still believe in the loss of blood as such, in the amount of blood drawn. The web of the foot of a curarized frog was burnt with a red-hot pointed needle, or with nitrate of silver." . . . "As soon as" . . . "slowing of the current and stasis had taken place, a leech was placed near the hock-joint." . . . "As soon as sucking began, the picture under the microscope changed in a striking manner. The blood current was immediately accelerated, blood-corpuscles which adhered to the

wall passed into the blood current, stasis was relieved, briefly, the inflamed capillary loops were in a short time, sometimes in a few minutes, entirely free, and presented in a few cases, at the termination of the experiment, the appearance of a perfectly normal and even accelerated circulation." The author was unable positively to determine whether the migrated white blood-cells "were in any way influenced by the bleeding." Owing to the tardy rate with which the blood was effused after scarification, the author stated that the good effects were not comparable to those of leeching. In like manner general blood-letting, by opening an abdominal vein, was inferior to leeching near the affected area. Dr. Gensmer considers it proven "that the antiphlogistic effect of local bleeding is due to a purely mechanical cause. Through the stronger current caused by the sucking of the leech (or by a cupping-glass, or by scarification) is the blood-corpuscle which adheres to the wall in the inflamed territory torn away, the obstructed capillary perfectly opened, and there is a normal, indeed a temporarily stronger, circulation established." . . . "Local bleeding prevents stasis. Bleeding not only does not cause local anæmia, but even occasions (of course transiently) arterial hyperæmia, that is, it causes a more intense flow of arterial blood to the inflamed point. Further, this abundant supply of arterial blood results in a better nourishing of the tissue; and that, therefore, the tissue is better able to withstand the effects of the inflammatory process is to be expected. It further follows that the antiphlogistic effect of bleeding depends upon the quantity of blood drawn, and that in the first place only the rapidity of the current is to be considered. Evidently the bleeding must take place, when possible, between the inflamed point and the heart, and not far from the former."[1]

What the effects of blood-letting are upon the general circulation has an important bearing upon the subject we are now discussing. According to the latest authority, "The water is

[1] Dr. Alfred Gensmer, Halle, Centreblatt für die medicinischen Wissenschaften, April 1, 1882.

increased, and the globules," . . . oxygen-carriers, "*are diminished* in relative amount." . . . "The action of the heart becomes *more rapid* and its force lessens" (the italics are mine), "the arterial tension falls." But how about the evil effects of blood-letting? "It is a remarkable fact, perfectly well known to old practitioners, and to which Sir James Paget has recently called attention, that the ill-effects of bleeding in healthy subjects, are very transient and easily repaired. The blood-globules, which are relatively more effected by bleeding than the other constituents, are quickly reproduced." . . . As surgeons we must all have had ample proof of this fact. Some years since, while pursuing other investigations upon the human blood, I observed the same fact, and noted it in the published report of my work. I need hardly point out, that if my experiments with those of Gensmer's are reliable, and the other facts quoted are really facts, our ideal remedy is found. From the quotations given of his paper, it is clear that our experimental results exactly tally. I think, however, that the superior advantages of leeching over scarification, etc., can hardly be similarly obtained in the human subject, since the relative size of the leech to the frog must have had a considerable share in effecting the good obtained by an actual aspiration of the blood. In my own experiments, by dividing a large vein in the tongue, I obtained the effect of blood-letting pure and simple. To produce the best results then, we should, when possible, bleed from one of the principal veins leading from the inflamed focus. When this is impossible, leeching or wet-cups should be resorted to, since, by both, the mechanical effects of aspiration are superadded to the mere outflow of blood. But many authorities maintain that blood-letting can be dispensed with in all cases, for exactly the same results are obtained by the use of arterial sedatives. To test the truth of such statements, it is only requisite to experimentally induce inflammation, and then give a large dose of gelsiminium. The arteries are seen to become smaller, the current slower, and if stagnation has already occurred, it increases or remains stationary. This and similar remedies then reverse that which both upon theoretical and experimental

grounds we have found to be most effectual. They certainly also interfere with the rapid absorption of effusion.

From a review of the facts set forth in this paper, I think I am warranted in stating the following propositions as the logical and practical outcome of my investigations:—

1. During the stage of dilated arteries, with increased rapidity of the current, but little danger of capillary changes with exudation need be apprehended, and here, perhaps, ergot, certainly arterial sedatives do good, either directly or indirectly, without blood-letting, by reducing the size and rapidity of the current, thus allowing the veins of the irritated area time to empty themselves even of an unaccustomed amount of blood. Thus, if vascular pressure changes have taken place, the vessels have an opportunity to return to the norm.

2. After stasis has occurred, or while it is occurring, weakening of the heart's action and a diminished volume of the current —*e. g.*, the effect of arterial sedatives—can do nothing but harm to the inflamed area, although, for the reasons given in proposition (1), it may prevent extension of inflammation in the circumjacent parts, which are merely in the earlier stages of congestion.

3. The results to be sought, and which are secured by local blood-letting, are, removal of the blood on the venous side, so that the vessels can not only empty themselves, but a certain amount of vis-a-fronte, *i. e.*, aspiration, is invoked: this secondarily results not only in a temporary return to the norm on the arterial side, but an increased rapidity (and here is an important point) —lessened force of the circulation. The acceleration of rate, without the weakened force of the circulation, would further damage the vessels, instead of which the increased rate of the current merely serves to sweep out the accumulated red blood-cells, the cause of the excess of oxygen, and the consequent cell migration. The vehement current, also, induces a rapid resorption of the effused liquor sanguinis, at once the stimulator to growth and the food of the cells. This latter advantage is not founded on theory alone, for it is a matter of common observation, that the mere amount of blood extracted produces no

sensible effects on an inflamed breast, for instance, *at first;* but, in a few hours, the skin, if carefully examined, has become wrinkled, and the organ shrunken. This effect is secondary to the loss of blood, and chiefly results from the absorption of the inflammatory exudate.

4. Arterial sedatives in the later stages are usually inadmissible, except as succedanea to blood-letting, as far as the focus of inflammation is concerned: the surrounding parts, which are merely congested, may be benefited by their exhibition. After blood-letting they act favorably, because, when the stasis has been overcome, they lessen intra-vascular pressure, and thus permit the bloodvessels to recover their normal condition. They also alleviate pain by lessening the bulk of blood in the part; *i. e.*, they relieve nerve pressure. As before intimated, this essay is, in reality, little more than a few notes on the effects of local blood-letting, and does not pretend to cover the extended field of either the local or general treatment of inflammation. If my remarks prove fruitful in the way of an instructive discussion, which may induce some of my hearers to resort anew to this useful but neglected remedy, I shall feel amply rewarded.

DISCUSSION.

Dr. E. M. Moore, of Rochester, New York.

I wish to express my gratitude to Dr. Nancrede for this paper. It is precisely the kind of paper I wanted to hear; and it seems to me that in it I have heard a little in advance of what we have been taught of the pathological process of inflammation. As a teacher of surgery, this question is always before me, and I have felt the difficulty of saying that we have anything like scientific knowledge of it.

One word now in regard to the non-use of the lancet. Although it seems to have gone too far, it appears to me, as we have gotten along so well without it, that it is in the interests of humanity and of true science not to return to the too free use of it. We find

great difficulties of diagnosis in many of the cases where the lancet was formerly used. If we could have more accurate diagnosis, we could make our therapeutics much more accurate, and thus secure the use of what is a very valuable agent in the treatment of disease.

In the early part of the century people were uniformly bled for pneumonia. It was so in the early part of my own practice. Then there arose a form of pneumonia known as typhoid. Dr. Rush's great reputation was largely dependent upon his advocacy of the lancet; and, unfortunately, he fell himself one of the first victims to its use in pneumonia, which had assumed an unusual condition. Louis has taught us the important fact, and we are still under the influence of his teaching, that a pneumonia may pass through all of its stages without any treatment whatsoever, and often with as good results as when under treatment, such as bleeding, tartar emetic, or indeed anything else. I had made no change in my opinion or practice until after an epidemic of pneumonia appeared in a neighboring town. In that epidemic every patient was bled, and every patient, without a single exception, died. Now, gentlemen, what a condition of things was this? I remember very well being asked as to the cause of this change from the treatment that had formerly been successful. My reply was simple: "I think that we have here a condition that is different from what we have had before, though with the same physical symptoms. Let us change the treatment at once. Let us give whiskey and quinine, etc., *ad libitum*." The first patient placed on this treatment recovered, and so with most of the others. It is such results as these that make us hesitate with regard to the use of the lancet. A more accurate diagnosis will enable us to do better in our therapeutics as science advances.

At another time an epidemic of cerebro-spinal meningitis appeared in Western New York. Of all the conditions in which we would be called upon to use the lancet, I think inflammation of the brain was formerly considered to be one in which its use was regarded as imperative. In this epidemic the physicians recognized the difficulty at once. They made their diagnosis, and made it correctly. They bled, and every patient died. We may be told here that from some cause during the early part of an epidemic it is generally more severe than later. However that may be, these physicians changed the treatment, and improvement in the results appeared at once. They began to give quinine and beef-tea, and

their patients got well. They reported it to me. I said: "Is it possible that you have such results with this treatment?" They replied that they did; and in order that I might confirm the diagnosis they requested my presence at a post-mortem examination of a patient who had died of the disease. The examination was made, and the spinal cord was found not only inflamed, but covered with an exudation of a fibrinous character. The diagnosis was correct. After this I was called to a neighboring town to see a boy suffering from the same disease. Six physicians had been summoned. The diagnosis was correctly made. They said: "The lancet must be used." The father begged that they might wait until I could be summoned by telegraph. On my arrival I related to them my experience in the cases to which I have alluded. They were surprised. They asked if they should adopt the plan of treatment without the use of the lancet. I told them they should, and they did so. While I do not think we should abandon entirely bleeding in inflammations, I think we should exercise caution in the use of a remedy of so much power.

Dr. Alfred Post, of New York, N. Y.

There are certain conditions with regard to inflammation in its very early stages that I think it is well for us to consider. One of the chief of these is the influence of position in retarding the access of arterial blood and in favoring the return of venous blood from the part. This is a fact which practical physicians are well acquainted with. We all know, that when an inflamed part is in a dependent condition, the inflammation is always aggravated. When now there is an elevated position given to the part, the pain and all the symptoms subside. I hardly think we can attach too much importance to the force of gravity in the treatment of inflammation, when it occurs in those situations in which we can avail ourselves of position.

There is another improvement in the treatment of the early stages of inflammation that is of value at the time when the symptoms first manifest themselves. This is by making equalized pressure on the part. We in this way keep the vessels comparatively empty and prevent stasis from occurring.

When inflammation has become thoroughly established, but in its early stages, I agree with the author of the paper that bloodletting is of the greatest importance, provided there is no asthenic condition of the system to countermand its use, either local or

general. There are certain forms of disease which seem to be positively asthenic in their conditions, and in them blood-letting cannot be employed. We have in scarlet fever a condition in which there is inflammation, but in it blood-letting is generally followed by fatal results. It is generally not well borne in erysipelas; and there are other asthenic forms of disease in which blood-letting is to be avoided. When inflammation occurs in patients of regular habits, who are well nourished, and from traumatic causes, I think that blood-letting is almost always of advantage.

A patient was brought to the Presbyterian Hospital, New York, about forty years of age, of good regular habits and in good general health. His head had been injured by a fall from the elevated railroad, and a day or two after admission he had delirium, indicating encephalic inflammation. He was so violent that he had to be bound to the bed. I asked the house-surgeon to bleed him. He said he had never seen it done, and asked me to do it. I bled him twenty ounces. He fell into a profound sleep, and the delirium did not return for three days. At that time the house-surgeon, who had been very favorably impressed by the result in the former case, bled him without waiting to consult me, and from this time the patient became convalescent. I have often been struck with the result of bleeding in puerperal peritonitis. I have bled the patient, when the pulse was small and wiry, before the disease had lasted long enough to enfeeble the vital powers. Within the first twenty-four hours I have found that, when the patient is bled from the arm until the pulse is fuller and softer, it was followed by relief. I do not remember a case in which the bleeding did harm. The patient, after bleeding, is put to sleep by a very moderate anodyne, and during the sleep the force of the disease is broken. That has been my experience. I do not speak of many of our hospital patients who are broken down by bad habits, but of patients with good habits, and in good general health. It is, however, a very doubtful remedy after the first or second day. There is then a state of depression of the vital powers which will not admit of blood-letting; but, in the early stage, my experience shows the recovery to full health is more rapid after the inflammation has been promptly arrested by blood-letting than where it has not been practised.

Dr. R. A. KINLOCH, of Charleston, South Carolina.

I am unwilling to let this subject pass with the apparent teaching of this body that blood-letting is the great remedy for inflammation. I am quite willing to confess that blood-letting has nearly always disappointed me in its results when used for traumatic inflammation. The discussion seems to indicate that as blood-letting takes away a certain amount of blood from the inflamed part, that, therefore, blood-letting is the remedy for inflammation. There are, I think, none of us in this room who will not admit that blood-letting does take away blood from the inflamed part, but does it relieve or control the essential and early phenomena of inflammation? The author of the paper, I think, did not clearly touch this question. How far does blood-letting overcome the essential part of the disease? No one doubts that bleeding at times relieves some of the symptoms. When blood is taken from a part, we may find that the severity of the general process is temporarily lessened by the blood-letting, but is the process arrested? If that has been proved, I have yet to know it. And I maintain that you may open a vein and give an outlet to the blood of the part indefinitely, and yet not arrest the destructive changes incident to the process. In other words, the bleeding does not control the essential nerve action, possibly reflex in kind, which is the initiation of the abnormal processes, and regarding which we are yet in ignorance, or with which we have only become partially acquainted.

Dr. MOSES GUNN, of Chicago, Illinois.

I think it is time for us to recall to our minds the fact that there are different kinds of inflammation. There is no doubt in many cases of traumatic inflammation that position will retard the flow of blood to the part and favor the returning circulation, and will exert a positive and health-giving influence in inflammations of that character. There is no doubt, either—and the older gentlemen know it—that there are certain forms of inflammation which are greatly benefited and even cured by blood-letting. This is a fact which the older members of the profession, I am sure, will not be willing to yield. There are other conditions also, as Dr. Moore has related, as in certain cases of specific inflammation, cerebro-spinal meningitis, for example, that generally will not admit of bleeding. Dr. Moore has stated that, in the epidemics of 1848 and other years,

generally fatal results followed blood-letting. I wish he had gone further and stated that they had not only fatal results, but that the blood drawn was deeply "buffed." I have seen cases in which, after blood was drawn, the corpuscles all rapidly sank to the bottom of the vessel, leaving a thick layer of fibrin on top—the characteristic "buffy coat" of the old writers—and yet the result of the bleeding was fatal.

There is no doubt, I think, that blood-letting, either local or general, may be of benefit. But the question raised by Dr. Moore still remains: Do we not need a better diagnosis? a diagnosis that will teach us to select the proper cases for blood-letting, and that will lead us to abstain from it in others.

Dr. W. T. Briggs, of Nashville, Tennessee.

I was much interested in the paper of Dr. Nancrede. His premises are well taken, and his deductions, based on actual experiments, are properly drawn. He has satisfactorily proved, by his well-conducted experiments, that blood-letting, local and general, is a remedy which may be used with benefit under certain conditions. I have always regarded blood-letting as a valuable remedy in the treatment of inflammation. But it, like all other means, must be used with judgment and discretion. If used indiscriminately, in all conditions and under all circumstances, disaster will certainly follow; but, if adapted to the condition and habits of the patient, if resorted to at the proper moment, without fear or hesitation, and if the operation is well done, good results will invariably follow. In young, active, strong patients, with sthenic inflammation in its early stages, no other remedy will act so promptly in arresting the pathological process and preventing its destructive effects. If the proper moment for the application of the lancet is allowed to pass, and the destructive effects of the pathological condition have already taken place, the lancet will do no good; its application under such circumstances will result in injury. I am fully satisfied that there is no remedy or combination of remedies that will do such good service in the treatment of inflammation as blood-letting when judiciously used.

I feel sure that much of the prejudice against the use of blood-letting in the treatment of inflammation has arisen from the lack of judgment on the part of the practitioner in adapting his remedies to the pathological condition of his patient, especially in the use

of local blood-letting, which is often substituted for general blood-letting when it should not be. If general blood-letting is indicated, local bleeding will not prove beneficial. For if local bleeding is resorted to while the heart and arteries are acting vigorously and strongly, no depletion or emptying of these vessels in the area of inflammation can be effective; the blood is supplied to the parts as fast or even faster than it is withdrawn. After the force and tension of the heart and arteries have been reduced, no remedy can effect the good results which local bleeding does, and there is no remedy which can take its place.

Dr. H. F. Campbell, of Augusta, Georgia.

Blood-letting has for many years grown more and more into disfavor, until now it is a disgrace for us to bleed under any circumstances; and even local blood-letting has been laid aside. Perhaps I do not bleed as much as I used to do; but, as a teacher of surgery, I am as careful as I ever was, and perhaps more so, to teach my students the use of the lancet as I am to teach them the use of the amputating knife, the lithotome, or any of the other instruments employed in the more brilliant operations, because I believe there are many cases in which blood letting is a good measure. In some cases it is the one remedy, and the one remedy indispensable for relief.

Blood-letting, as presented by our distinguished essayist, refers to local blood-letting. I think in some cases we may not know all the conditions of inflammation. It is better described than defined. The symptoms, heat, pain, redness, and swelling, were recognized many years ago. But we recognize them as those of true inflammatory action, due to the presence of too much blood in the part. The treatment is then to relieve the vessels and capillaries by incisions, by cupping, or by leeches. You relieve it by removing the blood from the part. You cause contraction of the walls of the vessels. The bandage may be applied, the limb may be elevated, or cold may be employed, with the idea of driving the blood from the part. To-day we hesitate to bleed in inflammations. In pneumonia we give beef-tea, brandy, and quinine, when formerly we bled in the same cases, apparently with equal success. It is a condition of weakness on our part to exclude bleeding under all circumstances. I have seen too much good result from the practice in the last forty years to ever give it up

entirely. I have seen older practitioners than myself who have gotten good results from it.

I cannot say too much about the value of the lancet in many inflammations. I will not condemn anybody for excluding it; for here is one fact I must confess, I have not bled one-half as frequently of late years as I did in the earlier periods of my practice.

Dr. E. H. Gregory, of St. Louis, Missouri.

I have been greatly interested in this paper. I cannot but believe that blood-letting is one of the most rational means of treating an inflammation. Certainly nobody can dispute the fact that there is in an inflamed part a local blood pressure. Nobody questions the fact that in inflammations the circulation is actually arrested when the inflammatory process reaches its acme. And it seems to me that nothing can be so rational as the direct lessening of the amount of blood in the part. Nobody can doubt that cases occur in which the taking of blood directly from the part is the best treatment. If taking of blood from a vein is better than taking it from an artery—very well, take it from that. Nobody can be precise in this regard. As for taking blood after the inflammation has been intensified to the point of stasis, it seems to me it may yet do good. We may thus cause the circulation of the inflamed part to move on after it has come to a halt.

Again, Dr. Kinloch has struck the key-note—has given the reason why we cannot nor do not cure an inflammation. Certainly there is no such thing as an idiopathic inflammation. Certainly there is no such thing as an essential inflammation. Certainly there is behind all inflammations a something; some cause which determines it. And we know that the cause which determines it is a necessity for repair. So far as we know experimentally, inflammation has but one object—repair. It only becomes a question of how we shall regulate the inflammatory process; because, if inflammation is a reparative process, its arrest prevents the accomplishment of its object. Repair is prevented. So that if we could only regulate inflammation, in a large proportion of cases, we would only get what is the object of the inflammation, the necessary repair of the part.

I am one of those who lived in the era of blood-letting. I might tell you of the success of my preceptor as a bleeder. I could go on and tell you of the success of the lancet in the early part of

my own practice. I could tell you of epidemics of pneumonia that were benefited by the lancet, and of those that were not benefited. I could tell you of patients with pneumonia who died when they were not bled. And I have seen cases in which the fatal result was as common when the lancet was withheld as Dr. Moore has seen when it was used. I am an advocate of general blood-letting when there is plethora united with fever and local inflammatory complications.

Dr. W. W. Dawson, of Cincinnati, Ohio.

I beg to say a word, to ask the Fellows who have spoken so enthusiastically for general blood-letting, why they do not adopt it, if it be such an efficient remedy. Does Dr. Gregory bleed his patients? Dr. Briggs commends it, but he has not, I venture to say, bled a patient within twelve months. (Dr. Briggs responded "yes, several times.")

You, Mr. President, doubtless, seldom use the lancet. (The President arose and said: "I see nearly all my patients in consultation after the time for bleeding has passed.")

I remember, Mr. President, listening several years ago (in 1875) to that classic address of yours on the "Lost Art"—bleeding—and we have heard something upon the same subject yearly since; but why do not physicians and surgeons practise what they teach? why not reduce their theory to practice?

Now, although our Fellow, Prof. Campbell, is so careful, he tells us, to teach his students the great value of the remedy, yet he confesses that he does not bleed frequently.

Why have we abandoned general blood-letting? Because we get better results from opium, rest, position, compression, local depletion, etc. It is a thing of the past.

Dr. C. B. Nancrede. I must return you my thanks for the kindness with which you have treated my paper, but I fear that, owing perhaps to the acoustic properties of the hall, I was in some instances not correctly understood. In the first place, I said that the diagnosis in all cases must be accurate as to the stage and character of the inflammation. I then said that the early stages of inflammation consisted of a rapid current, then slowing, a stasis of blood with a consequent accumulation of red blood-cells with the transudation of liquor sanguinis in consequence. There was

an excess of oxygen in the part which promoted the transmigration of the white blood-cells which plugged up the lymph-vessels so as to prevent the return of the cells which undergo various retrograde metamorphoses—pass into pus, if you please. Now, I said that, if you were able shortly after, or while stasis was taking place, to draw blood from the part, not only I but others have actually observed that the inflammation which we had taken hours to excite, disappeared and wholly went away. It was not theory; it was fact. We saw it under the microscope. As far as the human eye could see, these vessels had returned, practically, to the normal condition. *It is not theory*, I repeat, *it is fact!* Now I maintain that blood-letting in these cases is curative, for it gives the blood-vessels a chance to return to their normal condition. Therefore I say, if you wait a few hours for a consultation, the time is past. The moment the tissue changes have taken place, it is absurd to suppose that bleeding will enable the cells to return to the normal condition.

I speak, of course, of surgical inflammation; I do not speak of pneumonia or inflammation from any morbid influence at all. We presume that the tissues of the bloodvessels, in my case for instance, if I were wounded, are unaffected, for there is no influence to affect the condition of the bloodvessels.

Now, I know of pneumonia that when there is slight dulness, high temperature, pain in the chest, rapid pulse—in other words in the very onset of the disease—blood-letting will in a few hours, and suddenly, often cause everything to disappear. I know, too, that if we bled oftener in middle-ear inflammations, our friends, the aurists, would not have quite so many cases of chronic ear troubles. But, as for blood-letting, with the heart acting feebly and rapidly, the gentlemen will see that this is the very last thing I should do. I want a slight diminution in the force of the heart, but a quickening of its rapidity over the norm. I would then give digitalis to increase the power of the heart, but to only slightly reduce its frequency. If you are going to do anything you must do it at once.

We are very much afraid of the loss of the red blood-cells. Now, gentlemen, I have encountered great losses of blood in my experience where the cases did well, and I must say I do not very much fear the loss of a little of it. A twentieth part of the red blood-cells may be lost by the loss of a night's rest, or by a little diarrhœa, but they are all very quickly replaced. And I am very sure that all

that I have lost in making this little speech of mine to-day will very quickly be restored when I once get over to my hotel and have a little dinner.

Now, as to the question why we do not practise blood-letting. It is not the *fashion*, gentlemen. We have been taught that we must not bleed. I have, however, done it several times each year. In several cases, where large doses of morphia did no good, I have sat beside the patient and watched the blood flow, and have seen him drift off into a quiet sleep. We need not open the jugular vein to relieve the brain, we may open vessels in distant parts; by cupping over the mastoid region we deplete the radicles of the mastoid vein, *which empties directly into the lateral sinus*.

LISTER'S SYSTEM OF ASEPTIC WOUND-TREATMENT VERSUS ITS MODIFICATIONS.

By B. A. WATSON, A.M., M.D.,
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ALL important evolutions in nature, politics, and science are commonly foreshadowed by certain preliminary occurrences. The rising sun is heralded by the increasing light in the east. The approaching storm is proclaimed by certain atmospheric changes and the gathering clouds. Belligerent evolutions in political caldrons are reflected by the public press and the bickerings of the people. The primary effects produced by the Stamp Act and John Brown's raid may serve to illustrate conditions preliminary to war and bloodshed. An essential principle involved in the scientific treatment of wounds was discovered by Francisco Redi in 1668. Redi's experiments proved that maggots are not generated in dead flesh, but from eggs deposited by blow-flies. He was the first to give utterance to this remarkable hypothesis, "*no life without antecedent life.*" The work, begun by Redi, has been thoroughly elaborated by the experimental studies of Vallisnieri, Swammerdam, Reaumur, Schroeder, Dusch, Pasteur, Beale, Tyndall, and others. They have effectually succeeded in banishing the notion of spontaneous generation; and have likewise shown that fermentation and putrefaction *can only be produced by living germs.* Furthermore, they have pointed out the conditions which are requisite for their growth and multiplication, the various methods by which they are carried from one point to another, and also the means by which their destruction may be accomplished. In fact, the literature of this subject is very voluminous; but it is

thought to be unnecessary to enter more fully, at this point, on its discussion in connection with our consideration of wound-treatment. Allied to these discoveries, which we have briefly mentioned, were the observations made by physicians and surgeons in reference to the action of air on wounds. Galen declared, nearly eighteen hundred years in advance of his time, that the action of the air is often injurious and even dangerous to wounds, on account of the heterogeneous substances which it contains. Aerophobic ideas, similar to those expressed by Galen, were maintained by the following-named distinguished surgeons: Guy de Chaliac, Ambroise Paré, Magatus, Wiseman, Belloste, Pibrac, J. L. Petit, A. Monro, John Bell, Saucerotte, Dedalot, Lafflize, Champeau, Camper, Lombard, Boyer, and others. It was not until the commencement of the present century that the aerophobic ideas began to bear fruit which gladdened the heart of surgeons. The illustrious Delpech performed tenotomy subcutaneously for the first time, May 19, 1816. The object which he sought to accomplish was the protection of the wound in the tendon from the injurious action arising from contact with the air. He was perfectly successful. His success in this trial aroused in Athens a spirit of emulation, and we find Dupuytren performing subcutaneously myotomy in 1822. Stromeyer modified and improved Delpech's operation in 1831, and Dieffenbach published in the *Archives Générales de Médecine*, in 1835, an account of numerous and remarkable successes which he had obtained. Duval, Bouvier, and Jules Guérin contributed to perfect and popularize subcutaneous operations. The latter contended earnestly in favor of the noxious action of the air on wounds, against Malgaigne, Ollivier, and Velpeau, and finally had the satisfaction of partially convincing his opponents of the correctness of his views. The universal success which had attended the performance of subcutaneous operations, the absence of inflammation and suppuration, the great rapidity with which these wounds healed, had not failed to strengthen the opinion, which had been rapidly growing in favor, that the air contained some deleterious agent which exerted an unfavorable influence on all open wounds. In addition to all this, it had been ob-

served that simple or even comminuted fractures are rarely attended with danger to the life of the patient; although they are frequently attended by extensive subcutaneous lacerations; but so long as the principal bloodvessels remain intact, little or no anxiety is felt; nevertheless, it is well understood by all medical men that a compound fracture always endangers the life. Having now summarily alluded to some of the studies, experiments, and observations which were preliminary to the application of the germ theory to the treatment of wounds, it now remains for us to examine into the basis and character of Lister's treatment. This treatment is based essentially on the following conditions: First, the continued exclusion from wounds of all living germs, or the prompt destruction of the same in those cases where they have gained admission, thus preserving in the wound an absolutely aseptic condition until it has healed.

Second. Complete and uninterrupted approximation of the wound surfaces.

Third. The avoidance of all irritation or any disturbance of the relation of the wounded parts.

The most important objects sought to be accomplished by this system of treatment is the complete avoidance of all septic complications, the rapid healing of wounds by first intention or otherwise, the avoidance of all unnecessary danger to the life of the patient, together with as much relief from suffering as may be possible. The most important questions which now present themselves for our examination are, Does the Lister system of treatment fulfil these indications, and thereby secure the above-mentioned desirable results? It must now be admitted that every recognized scientific principle involved in this practice has been thoroughly examined, carefully studied, and fully recorded. It is, therefore, possible for any surgeon who is willing to devote a reasonable amount of time to the study of these records to form a just opinion of the value of these investigations, and the availability of the principles involved in the same for the treatment of wounds. It is not, however, my intention to convey here the idea that all these investigators have reached the same conclusions; but, notwithstanding this fact, it is certainly believed that

every honest inquirer after knowledge may readily discover the truth. It will be undoubtedly necessary for the student who desires to investigate this subject, to commence with the study of the principles involved in the Lister practice; and afterwards carefully analyze all the details of this system of treatment, and likewise its results. The scope of this paper does not enable me to even state the conclusions reached by individual investigators in their study of the principles of this treatment; but, I think, however, it must be generally admitted, and may be demonstrated to the satisfaction of any scientist, that it is possible to render *completely aseptic* nearly *all wounds* and to preserve them in this condition during the entire reparative process by the means employed in the Lister treatment. The accomplishment of this object depends, primarily, on the germicide property of carbolic acid as it is employed, and, secondarily, on the systematic manner in which it is used. It should be remembered that the Lister treatment of wounds is *not an empirical method*, but a *complete system of practice*, based on well-known scientific principles and facts, in which respect it differs widely from all its so-called "modifications." The principles involved in this treatment are now as thoroughly understood as those governing the construction and action of steam engines; and when the same degree of skill and care is exercised in the preparation of the material, and in the application of this wound dressing, as is required in the manufacture of this powerful motor instrument, there is no reason to doubt that the work accomplished by the former will be as satisfactory as that done by the latter. The fact is apparent to all, that the steam engine is only adapted to the performance of certain sorts of work; and it is equally true that the Lister system of wound treatment is limited in its applicability to certain classes of wounds. The surgeon, who would practise surgery successfully, must thoroughly understand the applicability of any wound dressing which he employs, and at the same time make himself familiar with the hygienic surroundings of his patient, as well as his idiosyncrasies, constitutional and local diseases, and the influence of the same upon the wound or injury which he is especially called to treat. Then, complications are

very frequently the cause of unfavorable terminations in cases of injury which otherwise would have been wholly unattended with danger. In order that he may be enabled to form a correct idea of the value of any local wound treatment, it is likewise necessary that the surgeon should recognize the existence of all wound complications, when such are present, since these frequently retard the healing process. The effect of unfavorable hygienic surroundings are certainly less to be feared when an aseptic system of wound treatment is strictly adhered to than under other circumstances, and the same is unquestionably true in some of the other wound complications. During the last fifteen years the high value of the Lister system of wound treatment has been fully attested. The theory on which it was originally based already has been supplanted by well-established facts and scientific principles. Experimentation and observation have established a new era in wound treatment. The old methods of wound treatment are no longer employed, except when modified or improved by engrafting on them certain parts of the Lister system. In one instance it will be found that the old treatment has been supplemented only by drainage-tubes, or the metallic sutures; but, in other cases, it will be observed that the modifications employed cause the treatment to approximate much more closely in its character to the Lister system. In fact, the majority of methods now employed are based essentially on the same principles as the Lister treatment, and it is generally claimed by those who employ them, that they are able to maintain by these means the required degree of asepsis and rest for the wound. Dr. Cheyne remarks on this subject, that: "We are now, therefore, able to take a much wider view of the meaning of the term 'antiseptic surgery' than is generally done. It is no longer surgery which only *excludes* the causes of putrefaction; we may now include under the term *all those methods of wound treatment in which, wittingly or otherwise, the growth and fermentative action of the lower forms of organisms* (*bacteria*) *are more or less impeded.* When we come to look at the numerous methods of wound treatment from this point of view, we shall see that there is perhaps none at present employed, with the exception

of the poultice dressing so much lauded by Mr. Savory, which does not, in some way or other, however imperfectly, interfere with the growth and fermentative action of bacteria, and which does not owe its chief virtues to that cause."[1] The strongest argument which can be offered in favor of the practice of Listerism is found in the fact that it has only required fifteen years to so far convince all the surgeons of Christendom of the correctness of the principles on which it is based, that they have, wittingly or otherwise, incorporated into their own methods of treatment, as the chief corner stone, the essential principles of the Lister system. Should additional evidence be required to establish the value of the Lister system of wound treatment, it may be found in Dr. Cheyne's *Antiseptie Surgery*, in which he has devoted two hundred and thirty-three pages to the consideration of the results of this system of practice. He has presented in this portion of his work a *convincing statistical report* from many of the most distinguished European surgeons. The high and well-earned professional character of each individual surgeon whose figures and operations have been quoted by Dr. Cheyne in connection with this subject, would be certainly entitled to a careful consideration in any Surgical Association, but the aggregated testimony of such a large number of eminent professional gentlemen forms a bulwark which cannot be readily overthrown by any power.

The vivid description given by Dr. A. C. Girard, United States Army, of his conversion to Listerism, is deemed worthy of a repetition in this connection, although it was originally published about seven years ago. He says: "During a sojourn abroad last winter my attention was particularly drawn to this innovation in surgery, as it has been introduced on the European continent but two years, and was the almost exclusive topic of conversation of the surgical profession there. It happened that my first intercourse was with some of the most decided and renowned opponents of the system, and I became acquainted

[1] Antiseptic Surgery, p. 206.

with all the objections to it before I had witnessed its advantages and benefits.

"I received, therefore, the glowing accounts of Lister's disciples with an incredulous ear, and it was only by travelling from one 'Lister Hospital' to another that belief in its superiority forced itself upon me. I became convinced, that, if it is not the only proper wound treatment, it is the safest one, and renders conservative surgery possible beyond what had ever been believed. It would take volumes to describe all that I witnessed, and I cite but a few examples. Who, before this, would have fearlessly opened the knee-joint for suppurative arthritis, as I saw done under the 'spray,' the patient recovering in a few days with a sound joint? Who would have expected an ovariotomy with general adhesions in a woman of seventy-five to heal in eight days without a symptom of reaction, or a laparotomy for the liberation of incarcerated peritoneal hernia in a moribund patient, healing in six days, or a resection of the ulna in nine days? Hospitals which had been in use for centuries, and had become hot-beds of infection, where the majority of operations, formerly, were followed by pyæmia, gangrene, and erysipelas, where everything had been tried to combat these evils, where treatment, 'open,' 'occlusive,' by 'immersion,' compresses of chlorine water, carbolized water, even Lister's 'gauze,' and 'paste' had failed, became entirely free from these complications as soon as Lister's system with *all* its precautions had been introduced."[1]

The adherents to the Lister treatment have steadily increased in number, in Europe, since Dr. Girard made his report; but there are some surgeons who still oppose it. This opposition is entirely maintained by those who have never practised the Listerian system, and their chief arguments against the practice appear to be based on the fact that in some instances organisms have been found under the "so-called Lister dressing," and that in some other cases a few Listerian surgeons have abandoned the use of the spray. The finding of micro-organisms in the secretions of an aseptic wound has been often questioned, but never absolutely

[1] Surg. Gen. Office, Circular Order No. 3, August 20, 1877.

disproved, although it may now be boldly asserted that observation has fully justified the conclusion, that, if they are ever found under these circumstances, they have certainly *lost their power to cause putrefactive fermentation*, and consequently they can never cause septic infection. In our own practice we have not always succeeded, even with the Lister dressing, in rendering wounds completely aseptic, and we should expect that a microscopical examination, in these cases, would reveal the presence of organisms. We, therefore, conclude that the presence of these micro-organisms in the secretions of wounds which have been treated antiseptically does not justify the assertion that this system of wound dressing is worthless.

We are now brought to the consideration of the abandonment of the spray by a few Listerian surgeons. We are aware that Mr. Keith has expressed a want of faith in its efficacy, even after having had a considerable experience in its use, while other surgeons have abandoned it for irrigation. The efficacy of the latter procedure cannot be questioned by those who admit the germicidal property of the carbolic acid solution in the strength in which it is commonly employed for these purposes. I desire here to call attention to the fact that the spray, when thrown on any solid substance, deposits within a few minutes a continuous layer of carbolic acid solution. It, therefore, follows that the spray having been properly directed on the parts where an incision is to be made, it becomes impossible for any germs to reach the wound without having first passed through the wet line, and, since this is continuous, no germs can pass or even remain in it many seconds without being thoroughly saturated with the solution. It is, therefore, certain that the spray, when judiciously employed, possesses the same efficacy as irrigation; but, that it may be abandoned if certain other precautions are taken to prevent the contamination of wounds during the period in which this agent is commonly employed, without materially affecting the value of the Lister system of treatment. It is thought that the strongest argument that can be urged against the use of the spray, is the fact, that it is always more or less annoying to the spectators, but this *frivolous objection* ought

never to be used against it in any case where it gives additional security to human life.

Having reviewed the principles, practice, and some of the objections which have been used against Listerism, we are now prepared to enter on the consideration of some of its so-called "modifications."

Prof. Thomas M. Markoe, who has suggested one of the most important modifications of the Lister system of wound treatment, introduces his subject in the following complimentary terms: "A verdict, almost unanimous, has been pronounced in favor of Mr. Lister's antiseptic method of treatment of wounds. In a practical point of view it is conceded to be an advance so great and so valuable that it is not easy to find its parallel even among the amazing strides of modern surgery. This favorable verdict has not been arrived at by the overshadowing influence of a great name, nor has it grown out of the teachings of a popular school of medical thought, nor indeed is it merely the result of a general acquiescence in a fashionable theory. It is the expression of the results of a fair and honest trial of the method by men of all schools and in all parts of the civilized world; a trial which, on Mr. Lister's earliest announcement of his views, was begun by a few earnest men with many doubts and with much hesitation; but a trial in which each succeeding witness confirmed the favorable testimony of those who had spoken before him, until, at the present moment, it would hardly be extravagant to assert that there are fewer men who do not acknowledge the practical advantages of Lister's method of treating wounds than there are who do not recognize the usefulness of splints in the treatment of fractures, or of the ligature in the arrest of hemorrhage. This unanimity, moreover, was not arrived at on the earliest experiences of the method, nor were the first trials in every case satisfactory, certainly not convincing. It has grown out of a large experience by competent men who have, after careful study, finally been convinced by what they have seen, and speak therefore of what they know. The indorsement of the leading surgeons in every capital in Europe is enough to establish a practice, of the results of which they would be ac-

cepted as competent judges."[1] The further perusal of this paper *cannot fail to impress the reader with the idea that Prof. Markoe is perfectly satisfied with the results of the Lister treatment*, and that his present efforts are mainly directed against the application of the germ theory to the treatment of wounds. The questions proposed and discussed in his article, which he has not *even claimed to answer*, show a strong bias in favor of the old antiphlogistic theory of wound treatment, and a prejudice against the more modern opinion on which Listerism is based. In speaking of the queries in connection with Listerism, he says: "I confess I do not think that all these interesting and important questions connected with the antiseptic method can at present be definitely settled, and that perhaps it is wiser to decide that statistics show for the Lister treatment a much larger proportion of success than can be claimed for any method preceding his discovery; but that as yet it is not clearly shown that his method is so superior to all the modifications of it that have been and are now being tried, as to vindicate its somewhat arrogant pretensions to be the true and only gospel of the surgery of wounds."[2]

The basis of the new departure from Listerism is described by its author in the following language: "The plan itself consists in the free and constant use of appropriate solutions of carbolic acid in water, no care being taken to keep the wound shut off from the influence of the atmosphere, and provision being made for the inner surface and cavities of the wound being constantly, or at least frequently, moistened by the carbolic acid solution. This is the outline of the plan, but its details can perhaps be best presented by describing its application to a case" in which the operation was performed for the removal of a dislocated and diseased astragalus. . . . "The wound, which consisted of a semilunar incision extending from behind and below the internal malleolus in a curved line over the dorsum of the tarsus to the external malleolus, was dressed by bringing

[1] Amer. Journ. of Med. Sci.; vol. lxxix. p. 305 *et seq.*, 1880.
[2] Ibid., p. 319, 1880.

the edges together with carbolized silk sutures, a long drainage-tube having first been passed across the deeper part of the wound from one malleolus to the other. This tube had been prepared so that the part within the wound had been perforated with a number of holes, care being taken that all that part of the tube which projected from the wound should be free from openings. The wound was then covered with the ordinary prepared carbolized gauze laid on in three or four thicknesses, first being well wetted with one-fortieth solution of carbolic acid, and so arranged upon the surface of the wound that the ends of the drainage-tube were free as they projected on either side. This we generally accomplished by cutting holes in the dressing opposite the points of entrance and exit of the drainage-tube, though sometimes the tubes could be brought out between the folded pieces of which the dressing was composed. This, retained in position by a few turns of the thin gauze bandage, also wetted with carbolic acid solution, completed the dressing. The limb was then suspended from a framework so that it was raised about six inches from the bed, and swung easily from side to side on every motion of the patient's body. This arrangement was found extremely comfortable, and permitted the drainage from the wound to be freely discharged from the lower opening. Four times a day a solution of one-fortieth carbolic acid was thrown through the drainage-tube with an ordinary syringe, and continued till the fluid discharged at the lower opening was perfectly clear. The result of this manipulation was that the fluid forced by the syringe into the upper end of the tube found its way freely into the cavity of the wound, distending it, and then found its way out at the lower orifice carrying with it all the fluid secretions which otherwise would have been more or less confined within the wound. This distension of the wound by the injected fluid would not of course happen to any great extent if the whole tube, including its lower orifice, were free from obstruction, but it very often happened that the outlet was impeded by plugs of dried pus or by clots, and then the penetration of the fluid to all the recesses of the wound was very complete—a penetration which we considered so desirable that sometimes the lower orifice of the tube was pinched

by the finger and thumb, while the injection was being made, for the express purpose of securing it."[1] We have now presented, in the author's own words, the essential parts of his method of wound treatment, which he has offered as a modification of the Listerian system, in order that we may justly compare it with that which he has styled, "the true and only gospel of the surgery of wounds." We have neither the time nor inclination, at this moment, to discuss fully the principles or theories on which these different plans of wound treatment are based. It should, however, be remembered that the Lister system of treatment aims to secure the *highest possible degree of rest for the wound*, and likewise *immunity from septic complications;* while Prof. Markoe's is directed especially against inflammation, as is shown by the following question, answer, and comments.

He asks: "Are there any agencies which will control the tendency to inflammation in wounded parts?" . . . "The answer of modern therapeutics would be, I think, unhesitatingly in the affirmative. Leaving out of consideration at present all those indirect influences of which every careful surgeon gladly avails himself, as rest, position, food, medication, regimen, etc., I contend that there are certain agencies which act directly, locally, and at times powerfully, in controlling or even in arresting inflammation. Of these agencies I recognize chiefly three, viz., cold, local blood-letting, and the action of certain drugs. Of the first two I have no occasion here to speak particularly." . . . "Of the last class of agencies which control inflammation I wish to speak more in detail; and for brevity's sake I will confine my remarks to one single drug, viz., carbolic acid. The testimony as to the singular power which this drug possesses of controlling inflammation is derived from various sources, and bears upon cases of inflammation attacking mucous and cutaneous surfaces, traumatisms and granulating wounds. Some of this testimony is popular and some is scientific."[2]

Three years have elapsed since Prof. Markoe published his

[1] Amer. Journ. of Med. Sci., vol. lxxix. p. 319 *et seq.*, 1880.

[2] Ibid., p. 312, 1880.

paper on "Through Drainage in the Treatment of Wounds;" and the question may now be properly asked *if his theory* of the action of carbolic acid has not been thoroughly exploded. What has been the experience of the surgical profession in regard to the action of a solution of carbolic acid, when applied in the ordinary manner, *i. e.*, as a douche, or on saturated compresses, or when acid has been mixed with some unctious substance, such as vaseline, and then smeared over the surface of an open wound? I have employed it, as above mentioned, in a large number of cases; and am, therefore, prepared unhesitatingly to affirm that *it will not prevent, or arrest inflammation* in such instances. Nevertheless, I am prepared to admit that the acid and the "through drainage," when employed as recommended by Prof. Markoe, exert a beneficial or restraining influence on inflammation; although it will *neither prevent nor arrest it.* It seems to me that the beneficial influences arising from this method of wound-treatment *can only be satisfactorily explained* by the more or less complete removal of the wound fluids from the wound, and a moderate degree of antisepsis exerted by the carbolic acid solution. It is a well-known fact that the wound fluids form the only *nidus* in which occurs primarily the multiplication of the micro-organism in these cases, and the germicidal property of carbolic acid has been thoroughly established. Furthermore, the fact that the Lister system of treatment, when properly employed, enables the surgeon to *prevent all inflammation in wounds*, must be accepted as a strong argument in favor of the antiseptic, and not the so-called "antiphlogistic" action of carbolic acid. The further study of our subject brings us to the consideration of the *great advantage arising from infrequent dressings* in all cases of recent wounds. Mr. Sampson Gamgee, F.R.S.E., has wisely remarked that, "Once a recent wound has been nicely adjusted, the less it is interfered with the greater the chances of healing. Under such circumstances, to meddle without good reason is to muddle."[1] It is very certain no experienced surgeon will attempt to refute the opinion expressed by

[1] The Treatment of Wounds, p. 9 *et seq.*, 1878.

Mr. Gamgee on this subject; and the importance of his views is never better illustrated than in amputations of the extremities, where the cut surfaces should be placed and kept in absolute contact with each other, if possible, during the entire healing process. The distension of these wounds by the impeded fluids ought never to be effected if it can be avoided, since it *seriously interferes with the healing process.* It would, therefore, appear that this proposed modification of Listerism carries with it its own condemnation in the following: "This distension of the wound by the injected fluid would not, of course, happen to any great extent, if the whole tube, including its lower orifice, were free from obstruction; but it very often happened that the outlet was impeded by plugs of dried pus or by clots, and then the penetration of the fluid to all the recesses of the wound was very complete—a penetration which we considered so desirable, that sometimes the lower orifice of the tube was pinched by the finger and thumb, while the injection was made."[1]

We are now satisfied that the majority of surgeons are prepared to admit that the *system or method of practice* which gives the most complete rest to the wound while it is healing, and at the same time the most complete immunity from septic complications, *is the one which will always yield the best results.* I have examined from these standpoints not only Prof. Markoe's modification, but the most of those which have been proposed, and I am now fully prepared to assent to the following statements made by Dr. Just. Lucas Championniere: "Perchance, in exceptionally favorable conditions, one may omit some parts of the dressing which are indispensable in bad surroundings; but the *ensemble* of the method must remain, and is demanded in the country as well as in town. Certain operations are justifiable only under its protection. Whatever has been the progress of modern surgery, the antiseptic method came at a time when the surgeon, often disarmed by surgical complications, saw his horizon limited by cruel uncertainties. It was impossible to predict the results of operation, and security was so imperilled

[1] Amer. Journ. of Medical Sciences, vol. lxxix. p. 321, 1880.

that certain conditions appalled the boldest, and certain operations were practically prohibited to the surgeons of cities; who, even though consummately skilful, found themselves unable to produce results equal to those of their far from expert brethren in the country, where the atmosphere is untainted. The antiseptic method came, and gave an impetus to surgery, for it provided skill with the means of success. This new power of surgery, it seems to me, can be acquired only by faithful disciples. . . . Approximations to antiseptic surgery, and attempts at imitating it, have everywhere ended in cruel disappointment. . . . But one can easily understand that, until it is acknowledged as a matter of surgical duty, only a small number will rigorously follow the necessary practice and attain satisfactory results. . . . Whatever I state I have tried and observed. I have educated myself upon all these points, and confident of success, I have fearlessly performed operations which formerly one would scarcely have ventured on. I assert more emphatically than ever, that we can do no better than to remain faithful to the words of the master."[1]

DISCUSSION.

Dr. J. H. Packard, of Philadelphia, Pennsylvania.

If I understood the essayist correctly, I must take exception to his statement that the majority of American surgeons have adopted Listerism. Certainly, on behalf of the surgeons of Philadelphia, I feel warranted in saying that it has not in their hands yielded such results as to induce them to adhere to it.

Dr. Alfred Post, of New York, N. Y.

I think I can say the same in regard to the surgeons of New York; I do not think that any of them now use the method.

[1] Championniere's Antiseptic Surgery. Translated and edited by F. H. Garrish, A.M., M.D., p. 14 *et seq.*

Dr. A. VANDERVEER, of Albany, New York.

I can say the same with regard to the surgeons of Albany and vicinity. In 1878 I published one of the first papers in this country on the death of a patient from poisoning by carbolic acid in a case of ovariotomy. And I desire also to call attention, in connection with Dr. Watson's statement with regard to the value of carbolic acid, to the fact that it occupies one of the lowest positions in the list of antiseptic agents, much inferior, for instance, to corrosive sublimate.

It is the through drainage that is the important factor in the treatment of wounds.

Dr. T. G. RICHARDSON, of New Orleans, Louisiana.

There is not a surgeon in our State who uses the Lister method.

The power of carbolic acid to control inflammation is, I think, very slight.

Dr. C. H. MASTIN, of Mobile, Alabama.

I wish to state that there is not a surgeon in the State of Alabama who uses the Lister method now. We have ceased to use it entirely.

Dr. T. A. MCGRAW, of Detroit, Michigan.

I had the misfortune some time ago to lose a patient from following out the Lister system; a loss which I ascribed to the poisonous effect of the carbolic acid.

The case was that of boy, on whom I performed an amputation at the knee-joint. I used a two per cent. solution of carbolic acid. After the operation I dressed the wound with adhesive plaster, then wrapped it in cotton and applied a bandage. I employed the method in detail. I left my patient in a good condition, free from pain, and resting well, with no fever, and the pulse good. In the morning I found him in a condition of coma, with contracted pupils and a rapid, feeble pulse, and from that time he sank, and in two days died.

Dr. W. W. DAWSON, of Cincinnati, Ohio.

You are, Mr. President, the apostle of antisepsis. You have taught, and still teach, that it is cleanliness. Listerism is still upon its trial. Is the better success we have the result of the use of the

germicides, or does it come from enforced cleanliness? One of our surgeons, Dr. Dandridge, has practised Listerism in all its completeness, and I doubt whether his success has been greater than that of surgeons who were ever watchful as to drainage, to ventilation, and disinfection.

To Dr. Markoe we are all indebted for the most thorough drainage. Under what is known as Markoe's "through drainage" wounds heal with unwonted rapidity. This is doubtless a step in advance.

Dr. H. F. Campbell, of Augusta, Georgia.

I believe that the action of the carbolic acid spray has the effect of retarding the suppuration in a wound, but I do not think that it is due to the influence of carbolic acid upon germs. I think that it arises from its effect on the reflex relations between the blood-vessels and the sensitive nerves.

Dr. David Prince, Jacksonville, Illinois.

In my opinion, we cannot be too thankful to Mr. Lister for directing the attention of the profession to the method of treating wounds so as to avoid the influence of those agents which we know float in the air. But the Lister system is so complicated in its details that it is doubtful whether it can retain its hold in practice when it is found that other and simpler methods will do as well. We cannot too much honor the man who stood at the head of the reform in the management of wounds that is now universally adopted not in detail but in general practice. If Listerism means the detail of the dressing of wounds, doubtless it is short lived, but if Listerism means the principles which are involved in antisepsis, Listerism is immortal. We know, Mr. President, that the open dressing of wounds, so that they shall be antiseptic, is as good as the closed method. The great end is to prevent putrefactive processes, and whether you attain this end by the use of carbolic acid, salicylic acid, iodoform, or corrosive sublimate, it is the same.

Dr. R. A. Kinloch, of Charleston, South Carolina.

I wish to express my faith, to a large extent, in the Listerian principles and the use of carbolic acid. It seems to me that we are here to-day, as American surgeons, expressing opinions which will

not be received very generally by surgeons throughout the world. As American surgeons it would appear from the discussion which has taken place that we are almost forced to say, that Lister had introduced a system capable of doing not much good, but a great deal of harm.

Dr. C. B. NANCREDE, of Philadelphia, Pennsylvania.

Having expressed my views on the subject of Listerism at the last meeting, I will not now occupy the time of the Association further than to say, that, having tried the method, I had abandoned it to ascertain whether my results were really attributable to the superiority of the dressing. My results were so much worse than with Listerian dressings, that I returned to it; besides, I have seen indisputable proof of the superiority of Listerism in the practice of one of my colleagues. I think that Dr. Packard is under some misapprehension when stating that no one in Philadelphia believed in the principles or adhered to the methods of Lister. Besides myself, there are a number of surgeons who either in some special operations or in all are Listerians.

Dr. B. A. WATSON. It seems to me that the gentlemen have mistaken entirely the tenor of my paper, as they do everything else that relates to Listerism. They appear to infer that the paper relates to the use of carbolic acid alone. The paper does not refer to the use of that acid alone, or to the use of any other single agent, but to the peculiar treatment which was perfected and introduced by Mr. Lister. Although carbolic acid may be condemned, yet the system which was introduced by him must be saved. Again, I think the gentlemen are mistaken in asserting that I said all the surgeons of the United States are advocates of the system. On the contrary, I know that Listerism in America has made but little progress; but, nevertheless, the present system of surgical practice has been modified to a very great extent by the introduction of the Lister treatment, and we find scarcely a wound treated in the United States to-day but what some part of Listerism is adopted. It is contended that some parts of it are good and some bad.

Thorough drainage was never practised to any great extent prior to the introduction of Listerism. Further, the great care and attention that is paid to cleanliness was not thought of until Mr. Lister

came forward with his system and practice. And when we object to this and that part of the Lister system, it seems to me that we are not objecting to Listerism as a whole. Some aim their darts at the spray and think that is the whole of the system; but the great objections come not from those who have tried Listerism, but from those who are willing to raise their hands and thank God that they have neither witnessed its application nor used it.

LIGATION OF THE PRIMITIVE ILIAC ARTERY FOR THE RELIEF OF SECONDARY HEMORRHAGE AFTER AMPUTATION AT THE HIP-JOINT.

By JOHN H. PACKARD, M.D.,
SURGEON TO THE EPISCOPAL HOSPITAL, PHILADELPHIA, PA.

[Read May 31, 1883.]

On the 23d of November, 1882, I performed a hip-joint amputation by Furneaux Jordan's method, on a young man named Rudolph Kavanagh, aged 23, a patient in the surgical ward at the Episcopal Hospital. Some months previously Dr. Simes, acting for my colleague, Dr. Hunter, had amputated the thigh at about the middle, on account of a tumor, and the stump had become the seat of osteo-myelitis.

I may state that an attempt was made to employ Davy's lever for the prevention of hemorrhage, but that it could not be effected; I, therefore, tied the femoral just below Poupart's ligament, as a preliminary measure; and the comes nervi ischiadici and other branches of the internal iliac, met with at the posterior part of the stump, were tied as they were cut, and as promptly as possible.

The patient did fairly well until the sixth day, when, as I was going through the ward, it was reported by the man set to watch him that there was some bleeding taking place. (There had been from time to time very slight hemorrhage noticed, but it was thought to be only from cutaneous vessels.)

Upon examination, the wound made for the ligation of the femoral artery was found full of clot, and some oozing was taking place. Thrusting my fingers down into the wound, I felt two or three small vessels pulsating, and these were easily tied. Still the escape of blood not only continued, but became

very free, and even alarming, when the fingers were removed from the wound. Pressure over the brim of the pelvis had no restraining effect upon it; and I therefore determined at once to tie the primitive iliac artery.

Committing the wound to the care of an assistant, I made an incision, found afterwards by measurement to be two and three-quarter inches long, a little above and parallel to Poupart's ligament, and with its mid-point about over the external iliac artery. Dividing the various tendinous and muscular layers, I readily felt the vessel, and with the fingers traced it up, pushing the peritoneum gently away, until I could feel the bifurcation distinctly. Going about an inch further, I cast a ligature about the primitive iliac without the least difficulty, and immediately closed the wound, leaving the ends of the ligature, of course, hanging out. In doing this, I did not *see* anything except the external iliac just at the bottom of the wound.

The patient had been greatly exhausted by anxiety and loss of even the comparatively small amount of blood which had flowed. He was kept lying with his head as low as possible, the foot of the bed elevated, and several hypodermic injections of sulphuric ether were given.

Quite free suppuration ensued, and for a few days the discharges from the wound of ligation had not only a fecal odor, but were of a brown tinge, so that there was reason to fear that the gut had suffered. But there was no sign whatever of peritonitis; in less than a week the discharge, although profuse, was simply thick yellow pus. The man gained steadily in every respect, being copiously fed and stimulated, and taking tonics in full doses. On the 18th of December, twenty days after the ligation, the ligature was found lying loose in the wound, which went on to closure at once.

At the present time this patient is looking extremely well, sitting up and going about in a wheel-chair or on crutches. The amputation wound is closed except a slight fistula, kept up probably by a few small spicula of bone yet to come away; and the man is desirous of going to his home.

A previous case, which occurred to me at the Beverly (U. S.

A.) Hospital, in 1864, was so like this in its general history, that I beg to present its leading features. Details of it may be found in the *Med. and Surgical History of the War of the Rebellion*, Part Third, Surgical Volume, and in the *New York Medical Journal* for December, 1865.

The man was 19 years of age, and received a ball through the head of the tibia, August 16, 1864. On the twenty-seventh day hemorrhage occurred, and amputation was performed through the lower third of the thigh. Thirty-five days afterward, bleeding occurred again, and the femoral was tied in Scarpa's space. On the 5th of November, the bone protruding, about four inches of it were removed by means of a chain-saw. Shortly after this, the stump became enormously swollen and painful, and was the seat of several abscesses. On the 19th of January, 1865, the bone was exposed, and found to be much enlarged, and in a state of necrosis as high up as the trochanters. The patient being already under chloroform, the femoral artery was exposed and tied in the upper third, and disarticulation of the hip performed by antero-posterior flaps. Some slight difficulty was experienced in securing the artery accompanying the sciatic nerve, but the amount of blood lost was not large. Extreme depression was exhibited for several days, during which the patient was kept lying on the operation-table, least an attempt at his removal might prove fatal. Large quantities of stimulants and concentrated food were administered, and the surface temperature was artificially kept up.

January 27th, bleeding again occurred, and the external iliac artery was tied by Dr. J. C. Morton, in my absence. The ligature came away in twenty-one days.

February 19th, the lower end of the divided artery poured forth blood, and was only controlled by direct pressure, which was kept up for about two weeks.

After this recovery progressed steadily, and by the end of March the man was well. In June he returned to his home in Maine.

It would be needless for me to go into any discussion of the statistics of re-amputation at the hip, which are now so readily

accessible to all interested in the subject; and I will simply dwell upon certain practical points which seem to me to find illustration in these two cases.

And first, as to the source of the bleeding. In both these cases the femoral artery was secured before the operation, and gave no further trouble; it was from branches of the internal iliac that the subsequent hemorrhage came. But in the second, the ligation of the external iliac was performed, and yet bleeding sufficient to endanger life ensued after the separation of the ligature; this I believe to have been, without any question, from the branches of the internal iliac; and no further trouble would have ensued had a ligature been placed upon the primitive iliac instead of upon its external branch, or if pressure upon the former vessel had been then instituted, as it afterward was with success.

I am not sure that it would not be sound surgery, before performing amputation at the hip, to apply a ligature to the primitive iliac, with the certainty that hemorrhage would thereby be most safely and effectively precluded.

I am well aware that cases have occurred in which the disarticulation has been performed with admirable success, bleeding being prevented by the use of either the abdominal tourniquet or Davy's lever. But I know also that damage has sometimes been done by these instruments, by pressure upon and bruising of the tissues of the bowel and the peritoneum.

As to the operation itself of ligating the primitive iliac, and arteries generally, I wish to say a few words: Writers on operative surgery have given the impression, and it is certainly widely prevalent, that in order to tie an artery it must be exposed to view. There are some vessels in regard to which this idea is correct; as, for example, the subclavian and axillary, which are surrounded by nerve-trunks which might readily be included in the loop, unless the isolation of the artery were insured by the aid of sight. I was present on one occasion when an experienced operator was in the act of tightening his ligature around a nerve-trunk instead of the vessel; one of his assistants felt and expressed

a doubt about the proper placing of the thread, and the error was corrected. But this narrow escape may serve to show that even with everything in full view the utmost care is needful.

Where, however, the vessel can be easily isolated, as in the case of the iliac arteries, the finger should be an amply sufficient guide to the operator. I know that it is the opinion of many surgeons, and I have often heard it expressed, that an inch or two more of wound of the skin makes very little difference; and this must surely be conceded, where by the larger incision the readiness and certainty of the operation itself are materially increased. But I submit that the best surgery is that which interferes least with the normal state of things: which goes most directly and with the smallest amount of exposure of the deeper parts to the accomplishment of its object.

Hamilton[1] directs an incision from just in front of "the free extremity of the eleventh rib, passing downward one-and-a-half inches in front of the anterior superior spinous process of the ilium, and terminating just above the internal ring by a sharp curve upward and inward of an inch or more, the whole length of the incision being about seven inches." He further says that "during the operation an assistant should stand upon the opposite side of the body, and with his hands in the wound sustain the peritoneum and the contained viscera. The surgeon must bear in mind also the danger of getting behind the psoas muscle while searching for the artery."

Fergusson advises an incision "from six to eight inches in length, almost parallel with the upper end of Poupart's ligament, and about an inch and a half nearer the mesial line; its lower end being from two to three inches below the anterior superior spinous process of the ilium;" a very vague and at first sight incomprehensible description.

But it would perhaps be hardly worth while to quote from various authors, as they all agree in advising that the incision made should be very much larger than is at all necessary.

One further quotation, however, I am tempted to make, from

[1] Principles and Practice of Surgery, 1872, p. 232.

a work[1] by a London teacher of operative surgery, Mr. Bellamy. This gentleman advises that an incision be made "from just above the middle of Poupart's ligament to a point an inch or so above and to the inner side of the anterior superior spine; or, commencing from a point close to the anterior superior spine, toward the edge of the rectus." This is vague enough; but the author goes on to say that after division of the muscles and transversalis fascia, "the peritoneal bag and its contents are to be pulled away toward the middle line, when the vessel will be seen just beyond the sacro-iliac synchondrosis."

But Mr. Bellamy further says, in reference indeed to the external iliac, but the same remark, if true, would be equally so of the common trunk: "When the peritoneum and intestines have been pulled away from the point at which the ligature is to be applied, it often happens that neither artery nor vein is to be found; in this case they have followed these structures, and will be discovered lying adherent to the under surface of the peritoneal bag, with the ureter, from which they must be cautiously separated."

Surgery like this, if practised on the living body, would scarcely be apt to effect much improvement in the statistics. I have tested the accuracy of Mr. Bellamy's statement on the dead subject, and cannot understand how the occurrence he describes can be possible even to the most ignorant and awkward operator.

Detached artery-needle.

I venture to ask attention to an instrument by means of which the passing of the ligature can be accomplished more easily than by the ordinary artery-needle. Probably most surgeons have, like myself, seen and experienced difficulty with the ordinary artery-needle, in getting hold of the ligature and disengaging it.

[1] The Student's Guide to Surgical Anatomy, Am. ed., 1874.

It occurred to me to employ what I call a detached artery-needle; simply a blunt needle, somewhat sharply curved, and roughened near its blunt point, so as to afford ready hold to a pair of forceps. This needle, armed with a ligature, can be grasped with a pair of catch-dressing-forceps, at any convenient angle, so as to make it right or left-handed, or straight forward, as may be desired. The needle having been passed under the vessel, the forceps are removed, and applied to the other end, drawing needle and ligature through without the slightest difficulty.

By the kindness of my friend, Dr. T. R. Neilson, who was present at the time of the operation on the primitive iliac, I am enabled to complete, as I believe, the statistics of ligation of that artery so far as they can be derived from the published cases up to this date. For the verification of some of the references, we must acknowledge the prompt and courteous aid received from the Surgeon-General's Office in Washington.

APPENDIX.

The following statistics are the result of a careful search into the rather wide-spread literature of the subject of ligation of the primitive iliac artery. Valuable aid in their preparation has been derived from a paper by Dr. Stephen Smith, of New York, entitled "A statistical examination of the operation of deligation of the Primitive Iliac Artery, embracing the histories, in abstract, of thirty-two cases," published in 1860 (*Amer. Journ. Med. Sci.*, vol. xl.). On comparison of the following table with this paper, it will be observed that one of the cases quoted by Dr. Smith, that of Deguise, of Paris (1840), has been omitted. This has been done on the authority of Chassaignac, who states that, in the case referred to, the *external iliac* was tied at a high point and *not the primitive trunk*.

In Prof. Agnew's *Surgery*, vol. i. p. 667, is a table containing sixty-three cases, thirty-two of them being those covered by Dr. Smith's paper. Of the additional thirty-one many are vaguely, and some inaccurately stated. The errors will be manifest on comparison with the table given below. The name of Gurlt has not been admitted for the reason that the reference given by Agnew (*Langenbeck's Archiv.*, B. iii. S. 96) is a translation by Gurlt of Dr. Smith's paper, above referred to, with a table of thirty-four instead of thirty-two cases, the two additional cases being those of Langenbeck and Bünger (see table). The translator makes no mention of having tied the vessel himself. Case No. 54 of Agnew's table, that of Watson, has also been omitted. The statements in regard to it are highly indefinite, and no reference is given, although the statement is made that "the abdominal aorta was afterwards tied." No mention of the latter operation is made by Prof. Agnew in his section on "Ligation of the Abdominal Aorta," in which several names of operators are given. Before striking this case from the list a careful, but unsuccessful, effort was made to find a reference to it.

A third alteration is the substitution of the name of Briarly for that of Hammond (Agnew's case, No. 52), which has been done for

reasons which will be apparent on reference to the notes placed beneath the table.

The operation of ligation of the primitive iliac artery is one which may fairly be claimed as of American orgin. First performed, but without success, in a case of gunshot wound, in 1812, by Gibson, it was done to the saving of life in a case of aneurism, by Mott, in 1827. The first operation in Ireland was done by Crampton, 1828; the first in Scotland, by Liston, in 1829; the first in England, by Guthrie, in 1833; the first in Russia, by Salomon, in 1837. Nélaton[1] quotes Mott's case as an exhibition of hardihood; and as his work was published in 1844, and he would certainly have known of any such operation performed in France, it seems likely that Chassaignac[2] is correct in his claim that he himself was the first of his countrymen to tie the primitive iliac, in 1850.

Below is a table containing sixty-seven cases. Those marked (*) will be found, as well as their proper references, in Dr. Smith's paper, *loc. cit.* Those marked (†) are contained in Dr. Agnew's table. Italic letters, in parentheses, placed after the name of the operator and date, refer to notes which follow the table.

[1] Élémens de Pathologie Chirurgicale, tome 1er, p. 501.

[2] Traité Clin. et Prat. des Opér. Chirurgicales, etc., Paris, 1861, tome 1er, p. 388.

Table of Cases of Ligation of the Primitive Iliac Artery.

No.	Operator and date.	Disease or accident.	Side.	Result: Recovery.	Result: Death.	Remarks.
1	W. Gibson, 1812*	Hemorrhage for ext. iliac artery	L.	..	1	Gunshot wound.
2	V. Mott, 1827*	Aneurism external iliac	R.	1	..	
3	Crampton, 1828*	Aneurism external iliac	..	..	1	
4	Liston, 1829*	Secondary hemorrhage	R.	..	1	Following amputation of thigh.
5	Mayo, (1832?)(a)	Iliac aneurism	L.	..	..	
6	Guthrie, 1833*	Tumor simulating aneurism	R.	1	..	Encephaloid tumor
7	Bünger, 1835†(b)	To prevent hemorrhage in removal of a tumor	L	..	1	
8	Stevens, 1836*	Aneurism external iliac	R.	..	1	
9	Salomon, 1837*	Aneurism external iliac	L.	1	..	
10	Garviso, 1837*	Hemorrhage from rupture of aneurism of external iliac	..	..	1	
11	Mouret, (1837?)(c)	Secondary hemorrhage	..	1	..	
12	Syme, 1838*	Aneurism external iliac	R.	..	1	
13	Pirogoff, 1838*	Secondary hemorrhage after ligation of ext. iliac for aneurism	R.	..	1	
14	Bushe, 1839*	To prevent hemorrhage from aneurism by anastomosis of labium	..	..	1	
15	Post, 1840*	Hemorrhage from an incision into an aneurism external iliac	R.	..	1	
16	Peace, 1842*	Aneurism external iliac	R.	1	..	
17	Hey, 1843*	Aneurism external iliac	L.	1	..	
18	Garviso, 1843*	Aneurism external iliac	..	1	..	
19	Stanley, 1845*	Tumor simulating aneurism	L.	..	1	Malignant tumor of ilium.
20	Barral, 1845†	Femoral aneurism	L.	..	1	
21	Luzenburg, 1846†	.....	..	1	..	
22	Lyon, 1847*	Aneurism external iliac	L.	..	1	
23	Gilpin, 1850(d)	Hemorrhage from traumatic femoral aneurism	..	..	1	Gunshot wound; death on 27th day.
24	Chassaignac, 1850*	To prevent hemorrhage in removal of an encephaloid tumor of thigh	L.	..	1	
25	Jones, 1851*	Aneurism external iliac	R	..	1	
26	Wedderburn, 1852*	Aneurism ext. iliac and femoral	L.	..	1	
27	Moore, 1852*	Tumor simulating iliac aneurism	L.	..	1	Encephaloid tumor
28	Uhde, 1852*	Hemorrhage from rupture of internal iliac in applying a ligature	L.	..	1	
29	Van Buren, 1853*	Aneurism external iliac	R.	..	1	
30	Langenbeck, 1855(e)	Hemorrhage from gluteal artery	..	..	1	Gunshot wound.
31	Edwards, 1857*	Rupture of an aneurism of external iliac	R.	..	1	
32	Holt, 1857*	Hemorrhage after ligation of ischiatic artery for aneurism	R.	..	1	
33	Meier, 1857*	Tumor simulating aneurism	R	..	1	Osteo-aneurism pelvis.
34	Parker, 1858*	Hemorrhage from stab in groin	R.	..	1	
35	Buck, 1858*	Hemorrhage after ligation of external iliac	L.	..	1	Femoral aneurism.
36	S. Smith, 1858*	Aneurism external iliac	R.	..	1	
37	Stone, 1859*	Aneurism external iliac	L.	..	1	
38	Goldsmith, 1859*	Aneurism external iliac	L.	..	1	
39	De Lisle, 1859†	.....	..	..	1	
40	Brierly, 1861(f)	Aneurism of gluteal artery	..	..	1	
41	Brainard, 1862*	Shot wound of pelvis	L.	..	1	
42	Syme, 1862(g)	Iliac aneurism	L.	1	..	
43	Cock, 1863†	Iliac aneurism	R.	1	..	
44	McKinlay, 1863†	Secondary hemorrhage following amputation of thigh	..	1	..	
45	Bickersteth, 1864†	Aneurism of iliac artery.	R.	1	..	
46	Ingram, 1864†	Hemorrhage from shot wound of thigh	..	..	1	

TABLE OF CASES OF LIGATION, ETC.—*Concluded.*

No.	Operator and date.	Disease or accident.	Side.	Result: Recovery.	Result: Death.	Remarks.
47	McKee, 1864†	Shot wound	L.	..	1	
48	Cutter, 1864†	Anterio-venous aneurism of external iliac artery and vein	..	..	1	
49	Isham, 1864†	Traumatic aneurism	R.	..	1	
50	Hamilton, 1864†	Shot wound of pelvis	..	..	1	
51	Hargrave, 1864†	Iliac aneurism	L.	..	1	
52	Baxter. 1866†	Aneurism of aorta	R.	..	1	
53	D'Almeida, 1867† (*b*)	Inguinal aneurism	..	..	1	
54	Maunder, 1867†	Inguinal aneurism	R.	..	1	
55	Pitta, 1868†	Aneurism ext. iliac and femoral	L.	..	1	
56	Busch, 1870†	Hemorrhage from wound of external iliac	..	..	1	
57	Czerny, 1870†	Shot wound in upper part of femur	R.	..	1	
58	Ladureau, (1871?)†	Spontaneous aneurism	..	..	1	
59	Baker, 1871†	Abscess from sacro-iliac disease	..	..	1	Supposed aneurism
60	Gouley, about 1872†	Aneurism external iliac	..	..	1	
61	Barbosa, 1873†	Femoral aneurism	L.	..	1	
62	Carr, 1875†	Hemorrhage after ligation of femoral for popliteal aneurism	..	..	1	
63	Caldas, 1876†	Aneurism external iliac	L.	1	..	
64	Hunter. 1877†	Femoral aneurism	L.	..	1	
65	A. B. Mott, 1877†	Femoral aneurism	..	..	..	
66	Nicoladoni, 1881 (*f*)	Iliac aneurism—traumatic	R.	..	1	
67	Packard, 1882	Secondary hemorrhage	R.	1	..	
		Total	..	14	51	= 65
		Result not stated	..	..	..	2
						67

(*a*) *Mayo:* Lond. Med. and Surg. Journ., 1832, IV. p. 349.

(*b*) *Bünger:* Exarticulation of a tumor (fibroid?) of left upper thigh, which was attached to the whole side of the pelvis. Ligation of primitive iliac. Death from shock during the following night. Langenbeck's Archiv f. Klin. Chirurg., B. III. S. 96. (Gurlt's table.)

(*c*) *Mouret:* Journ. de Conn. Med.-Chir., Paris, 1838–39, VI. p. 237.

(*d*) *Gilpin:* New Orl. Med. and Surg. Journ., 1851–2, VIII. p. 598.

(*e*) *Langenbeck:* Ligation 30 days after wound was received. Death, from shock, 10 hours after operation. Langenbeck's Archiv, etc., B. III. S. 96. (Gurlt's table.)

(*f*) *Briarly:* Evidently the same case that in Agnew's table is assigned to Hammond. The reference is an editorial in the San Francisco Medical Press, April, 1861, vol. ii. p. 92, which states that "Dr. Wake Briarly" then "late Professor of Surgery in Washington Medical College, Baltimore, and Dr. Hammond, tied the common iliac artery for aneurism of the gluteal artery of nearly three years' standing." Patient lived something over four weeks. No details of the operation, no date, no information as to the cause of death, nor report of an autopsy are given.

(*g*) *Syme:* "A case of iliac aneurism remedied by opening the sac, and tying

In order to obtain satisfactory deductions from the above table, it is necessary, of course, to group the cases into classes, which, after Dr. Smith's example, we will do as follows:—

I. Those cases in which the operation was done for the *arrest of hemorrhage.*

II. Those in which it was done for the *cure of aneurism.*

III. Those cases in which *tumors simulating aneurisms* led to its performance.

VI. Those in which the vessel was secured *to prevent hemorrhage during the removal of a morbid growth.*

Of the *first* group (*for hemorrhage*), we have twenty-two cases. Of these, 3 recovered, 19 died. *Mortality*, 86.36 per cent.

To the *second* group, thirty-five cases belong; of which, 9 recovered, 24 died, and in 2 cases the result is not stated. *Mortality* in 33 cases 72.72 per cent.

For tumors simulating aneurism (Group III.) the vessel was tied in five cases, one of which recovered, four died. *Mortality*, 80 per cent.

Three cases belong to the *fourth* group, of which all died.

The general mortality for the sixty-five cases in which the result of the operation is stated in our table, is 78.46 per cent.

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the external iliac, the internal iliac, and the common iliac arteries." The Lancet, 1862, vol. i. p. 602.

(*h*) *D'Almeida:* Gaz. Médicale, Oct. 3, 1874.

(*i*) *Nicoladoni*, of Innsbruck; A case of a large false aneurism following a stab in the right groin. The tumor began at the level of the anterior superior spinous process of the ilium, extended across the median line, two inches below the umbilicus, and extended downwards to behind the pubis. An effort was first made to secure the epigastric artery, under the belief that it was the source of mischief. The operation consumed three-and-a-half hours. Death followed in forty-five hours from the time of the operation. Post-mortem examination showed beginning peritonitis. Wiener Medizinische Presse, 1882, XXIII. p. 234.

In addition to the foregoing, I have found the following references, but, having been unable to get access to them, have not included them in my table. They, as well as cases marked (*a*) and (*c*), have been verified through the courtesy of Dr. J. S. Billings, Surgeon-General's office:—

Beugnot: Journ. de la Soc. Méd. de la Nouv. Orleans, 1859–60, I. p. 242.

Dumreicher: Wiener Med. Wochenschr., 1854, IV., p. 186.

Lemprière: Austral. Med. Journ., Melbourne, 1864, IX. 7–15.

Medoro: Ann. Univ. di Med., Milano, 1832, LXIII. p. 172.

DISCUSSION.

Dr. W. C. B. Fifield, of Boston, Massachusetts.

I admire, as much as any one can do, the great skill and courage displayed by Dr. Packard in his operation. But if I understand the operation of Mr. Furneaux Jordan, I fail to see the necessity of Dr. Packard's first procedure. I fail to see the necessity of the primary ligation of the common femoral or of the primitive iliac artery. It seems to me that the great advantage of Mr. Furneaux Jordan's operation is, that it enables us to dispense with all these preliminaries. As I understand the operation of Mr. Furneaux Jordan, an incision is made down along the outer side of the thigh; the tissues are then dissected from the bone, the vessels are ligated as they are cut, and the head of the bone is removed from its socket. During the last winter I saw an operation of this kind performed at at the City Hospital, Boston, by one of my colleagues. It was a case in which there was an enormous cancerous tumor involving the knee-joint. The patient was in such a state that it was doubtful whether any operation could be performed, but it was finally decided that the amputation should be made in the middle of the thigh. The method selected was the circular one. At the end of two weeks, however, it was found that the disease had reappeared in the stump, and it was decided to re-amputate. No tourniquet was used, no pressure was made upon the femoral, nor any other procedure employed for the prevention of hemorrhage. The incision was quickly made on the outer side of the thigh down to the bone, and the vessels were ligated as they were divided. The tissues being dissected from the bone, the wound was filled full of sponges, the bone was disarticulated and removed; and, as far as I know, the man did well. I have never heard of any secondary hemorrhage in his case.

As regards the application of ligatures to arteries by feeling, I believe that the old teachers of France used to instruct their students to put ligatures around arteries while looking toward heaven, without looking at the artery at all.

So far as ligature of the primitive iliac artery is concerned (although I am not now sure whether it was the internal or primitive iliac), I remember it was performed by Dr. Bigelow, upon his

first return from Europe; and I remember that he used a dark lantern to throw a strong light upon the artery, but I believe it was not successful.

Dr. Packard. I am not at all surprised at what Dr. Fifield says. I presume most of the surgeons here have seen just such cases. But I thought it safer to put a ligature on the vessel. After the operation, we had nothing to do but to ligate the vessels at the posterior side of the stump. We had some difficulty in tying one of these in the fossa occupied by the sciatic nerve. I will venture to say that these vessels were thoroughly tied, and the man was carefully watched, and yet hemorrhage occurred in spite of it all. Now I do not see why it is any better to stuff the wound with sponges or to take the risk of an assistant's hand slipping off the artery than it is to tie the vessel and be sure. It seems to me that it is better to prevent hemorrhage by every method that is in our power. I cannot see why we should prefer secondary ligation, after stuffing sponge into the wound, to primary ligation.

As to the operation with our eyes toward heaven. I believe that it is always a good thing for us to cast our eyes that way, but was not aware that any French writer had ever taught that method of ligating arteries. Most writers say "the artery can be exposed," or "the artery can be brought to view" after such and such procedures. But, if you can lay the external iliac artery bare and pass the finger in and trace the vessel up, you can perceive whether the ureter is in the way, and there are no large nerves concerned, so that there is no need of seeing. And there ought not to be any difficulty in passing the needle between the artery and vein.

Dr. P. S. Conner, of Cincinnati, Ohio.

There are cases, it seems to me, in which a modification of Mr. Furneaux Jordan's operation may be made with advantage. In a case under my own observation, about two years ago, I employed his method in an operation. The case was one of necrosis following gunshot injury attended with an epitheliomatous ulceration. I made an amputation at about the junction of the middle and upper third, using the Esmarch bandage. I tied the femoral and a few small vessels, but upon examining the bone, found that the condition of the medulla was not such that I was willing to leave it. I therefore cut down upon the outside and disarticulated the bone

—not a single vessel required ligation. The patient made an excellent recovery with the exception of a small sinus, which I, at a later period, scraped and cauterized. When I saw him a few months ago, recurrence of the epitheliomatous condition had taken place. In my judgment, the tying of the artery is the wisest thing that can be done in cases where the operation is so high up that the tourniquet or Esmarch's bandage cannot be well applied.

Dr. Fifield. I made an assertion with regard to the statements of French writers on anatomy; Dr. Packard makes a contrary assertion. If I know anything, I know that M. Faraboeuf gives the method of tying the artery by touch, and gives as the reason, that the surgeon ought to be able to distinguish the artery by the feel, by the rolling under his finger. I did not state that Dr. Packard was timid, but it does seem to me that the primary ligation of the femoral or external iliac artery is an objectionable procedure in this operation, because everybody knows that when we tie an artery and the ligature separates, it is then we are liable to have secondary hemorrhage. Now then, unless I very much misunderstood the case, I think the hemorrhage came from the ligature which had been primarily applied to the femoral artery. Whether the hemorrhage came from the neighborhood of the wound made for the ligature of the common femoral or from the wound behind and outside, the author of the paper did not say; but I had the idea from what he said that it came from the point where the ligature separated from the common femoral.

Dr. Packard. I had no intention, in my remarks a few minutes ago, of making a contrary assertion to Dr. Fifield. I should have said, that I did not know of any French author who directed tying the artery by touch and not by sight. I am only acquainted with M. Faraboeuf's work by looking at certain places in it, and I was not familiar with any advice of that kind.

As to the source of the hemorrhage. I would say that, after opening up the ligation wound and passing my finger in to trace the source of hemorrhage, my finger passed on back into the posterior flap. It did not, therefore, at the moment, seem to me possible to do anything but tie the primitive iliac. Now I believe it is less dangerous to tie the common than the internal iliac, and therefore

I thought that offered me the best chance of saving the man's life. And it would be simply when I anticipated difficulty in the control of hemorrhage, or possibly where I had insufficient assistance, that I would apply a ligature to the primitive iliac, or in a similar case where hemorrhage had taken place, as in the case in the Beverly Hospital before mentioned.

As to the Furneaux Jordan operation, my impression was that the main features of it were the direction and extent of the incision. But I had forgotten that he directed particular attention to anything else than the incisions, cutting an abundant flap and allowing the vessels to be secured without difficulty. But I do not see how, when hemorrhage occurred after the ligation of the common femoral, it could be reached in any better way than by the ligation of the primitive iliac.

I had spent as much time as I thought I was justified in doing. There was a gush of blood which made me feel that the man would die immediately if I did not do more than would be secured by ligation in the wound. I was working at the bottom of the amputation wound. I suppose I might have opened the wound, but in doing so, I should have disturbed other vessels in the neighborhood, and I felt that I should be more secure in dealing at once with the primitive iliac.

THE USE OF THE TREPHINE IN TRAUMATIC EMPYEMA ASSOCIATED WITH THORACIC FISTULA.

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[Read May 31, 1883.]

Chronic suppurative pleuritis with an imperfect fistulous outlet, either external or bronchial, is not an uncommon result of gunshot or other penetrating wounds of the thoracic cavity; and it is no secret that the resources of surgery have not heretofore offered much encouragement to the unfortunate sufferers. In the majority of such cases there is contraction or "sinking in" of the injured side from well understood inflammatory changes within, a constant discharge of fetid pus, persistent cough, irritative or septic fevers, and more or less rapid exhaustion of strength, terminating usually in death within a few months, or a year or two at the farthest.

Two main difficulties are encountered in the treatment of such cases: 1st. Imperfect drainage, upon the correction of which the life of the patient depends; 2d. Permanent separation of the lung from the chest-wall by contraction of the organized exudative membrane upon the surface of the former. It is to these two points I desire to call attention.

1. The serious obstacle to drainage in these cases is not the ordinary stenosis to which nearly all sinuses are liable, but approximation of the ribs consequent upon the "sinking" or "falling in" of the chest-wall. Owing to the shortness and greater degree of fixedness of the first four or five ribs very close approach of their adjacent borders is seldom seen, except in quite young subjects. The same is likewise true throughout

the entire series near their spinal attachments. But in other situations, where greater latitude of motion exists, more especially along the lateral planes of the thorax below the fifth or sixth ribs, it is not rare, in the cases now under consideration, to find the adjacent edges closely applied, and sometimes even slightly imbricated. Under these circumstances it is impossible by any ordinary means to preserve satisfactory drainage of the suppurating sac by an opening, however extensive it may have been made, in an intercostal space. Tolerably strong silver canulæ (objectionable on other accounts) have been indented by the approaching bones, and rubber tubing is worse than useless.

The effect of the entrance of air into undrained pyogenic sacs is familiar to every one, and in no other part of the body, not excepting pelvic and psoas abscesses, is the decomposition of the contained fluid more complete, or more certain to result in the death of the patient by the production of pyæmia, or, more slowly, by consuming irritative fever.

2. The obstacle to the closure of the pus cavity arising from compression of the lung by inflammatory membrane, and the consequent inability of this organ to expand to its original dimensions, is not of itself detrimental to the life of the patient. Pyogenic surfaces, when freely exposed to the air, and, at the same time, sufficiently protected to prevent desiccation, more especially if kept moist by an antiseptic fluid or vapor, do not usually give rise to pyæmia or irritative fever. They are certainly sources of great discomfort and annoyance to the patient, but fortunately, in the cases now under consideration, they are often closed by the same operation, by which the difficulty of drainage is overcome, as I shall presently show.

Seeing, then, that the great danger in these cases depends mainly upon the want of suitable drainage, it is to the best method of effecting this purpose that I desire to draw attention. The impracticability of securing a sufficiently free opening through one or more of the intercostal spaces, especially along the lateral and lower parts of the thoracic wall, has just been referred to. And I may here add that the same objec-

tion applies to a counter-opening as well as to the original outlet. Under these circumstances the surgeon's only resort is removal of a portion of one or more ribs. This may be accomplished by one or other of the ordinary methods of bone-resection, but, in my opinion, much more readily, and, in most cases, with equally good results, by the use of a large trephine.

The idea of trephining the thorax is not new. It is said to have been proposed by Hippocrates, and in more modern times has been modified by Reybard, and adopted by Récamier, Trousseau, and others,[1] in idiopathic empyema. This operation consisted, however, in simply perforating a rib, and inserting a canula, the greatest care being taken to prevent the entrance of air.

Lossen, of Heidelberg, ascribes the first suggestion of resection of the ribs, for what he terms retro-costal abscess, to Roser in 1859, and states that this surgeon performed the first operation of the kind in 1865, with the effect of curing his patient in fourteen days. He (Lossen) adds that, in 1869, Simon excised a portion of the sixth rib, in a case of empyema with fistula, for the purpose of permanently enlarging the canal. The cavity ultimately closed, the favorable result being due, in the opinion of the operator, to sinking in of the resected rib.[2]

Dr. Schneider, of Königsberg, in a case of pleuritic suppuration resulting from gunshot wound involving the third rib, removed from the second rib a section 2 inches in length, from the fourth 3.8 in., from the fifth 3.8 in., from the sixth 4.4 in., and from the clavicle 1.5 in., by which means the chest-wall was allowed to sink in and obliterate the pleural cavity.

It will thus be seen that the benefits derived from resection are threefold: 1. Unobstructed drainage; 2. Free space for the application of antiseptic remedies; 3. Shrinkage of the chest. In a large number of cases, more especially those in which the collection of pus is confined to the lower and lateral regions of the thorax, the mobility of the ribs, the flexibility of the long

[1] Traité de Pathologie Externe, par Aug. Vidal (de Cassis), tome iv.
[2] London Medical Record; Amer. Journ. Med. Sciences, July, 1878.

costal cartilages, and the yielding nature of the diaphragm permit the closing of quite large retro-costal cavities without any assistance at the hands of the surgeon other than what may be necessary for drainage and asepsis. In such cases a large opening at the site of the fistula, or, if needs be, at a more dependent point, is all that is required. For this purpose the application of a trephine with a crown sufficiently large to embrace the entire breadth of a rib commends itself as the readiest and safest method, and does not hinder subsequent resection of the adjacent ribs, if such procedure should become necessary.

So far as I have been able to ascertain, this operation originated in New Orleans, and is almost peculiar to that city, where it has been frequently performed during the past twenty years with increasing favor. Indeed, it is no longer a rarity there, and those who resort to it do not deem it necessary to keep detailed records of their cases. This neglect is much to be regretted, but thouhg defective in statistics, I trust that my presentation of the subject may not, on this account, be discredited. To the late Professor Warren Stone, Sr., M.D., is undoubtedly due the credit of having first performed this or any other method of resection of the rib for empyema, as the following history will clearly prove:—[1]

Case.—Charles W., aged 17 years, was stabbed in the back with a large pocket-knife, in the hands of one of his college-mates, at a well-known institution in North Carolina, November 19, 1860. The wound penetrated the thoracic cavity about two inches to the right of the spine between the fifth and sixth ribs. Pleuro-pneumonia was the result, and very soon offensive grumous pus began to discharge from the opening. After several weeks' confinement to bed the patient rallied sufficiently to be taken to Paris the fol-

[1] Since this paper was read, my attention has been called by Professor S. W. Gross to an article in the *British Medical Journal*, January 21, 1860, entitled, "Case of Traumatic Empyema of sixteen months' standing with Fistula, treated successfully. By Albert G. Walter, Fittsburgh, Pennsylvania, U. S." The case was a knife wound, resulting in retro-costal abscess which opened spontaneously. December 8, 1857, one inch of the seventh rib was removed with bone-pliers. To secure better drainage two inches of the eighth and ninth ribs were removed in like manner, February 2, 1858, followed by injections of tincture of iodine. On January 1, 1859, the patient was reported entirely well.

lowing summer. Here he was placed under the professional care of MM. Velpeau, Nélaton, and Maissonneuve, who declined to do anything more than to attempt to dilate the fistula and cleanse the cavity. No benefit having accrued after several weeks' treatment, and no encouragement to remain having been given, the patient was returned to die at his home in Mobile, Alabama.

In February, of the following year (1862), he was brought by his parents to New Orleans, and admitted into the private infirmary of which Professor Stone and I had charge. At this time he was emaciated to an extreme degree, racked by cough, and exhausted by irritative fever. The right side of the chest was somewhat contracted, and from a very small fistulous opening at the site of the wound fetid pus was slowly exuding. A careful examination led to the conclusion that there was a considerable collection of pus in the pleuritic sac, notwithstanding the diminished size of the affected side. To get rid of this poisonous fluid, establish free drainage, and render the cavity aseptic, were the immediate indications, but how to accomplish these ends, after the signal failure of the three most distinguished surgeons of France, was the question. The problem was soon solved, however, by my distinguished colleague, when, after contemplating the situation for a few minutes, he turned to me and asked what would be the objection to enlarging the fistula by a trephine applied to the adjacent rib below. The suggestion met with a hearty approval, and, with the crown of a medium-sized instrument, a disk, involving the entire breadth of the bone, was immediately removed. Still the cavity was not opened, in consequence of the presence of the densely thickened pleura, by which it was lined. To divide this freely, despite the possible wounding of the intercostal artery, was the work of a moment, when out gushed an immense stream of decomposed pus, so disgustingly offensive in its smell as almost to drive every one from the room. After the flow had in a measure ceased, and fifteen or twenty ounces had escaped, the cavity was washed with a tepid solution of chlorinated soda, and a small roller-bandage thrust into the wound to serve both as tent and plug. No hemorrhage followed the operation, and I have since been convinced by additional experience that, in such cases, the intercostal arteries in the immediate vicinity of the fistula are usually obliterated by contraction of the fibrinous deposit. The plug was subsequently removed twice a day, and the cavity freely injected. The patient's health

began to improve immediately, and in less than a month he was able to go unaccompanied wherever he desired. In the mean time the sac was undergoing steady diminution in size by expansion of the lung and subsidence of the chest-wall, and we indulged the hope that it would ultimately be obliterated. Unfortunately its main portion was below the level of the opening, and at each dressing a considerable quantity of pus was made to flow out by placing the patient upon his back near the edge of his bed.

For reasons not necessary to mention, the patient left the city soon afterwards, and the following year (1863) was sent with dispatches from the Confederate Government to Europe, where he continued the treatment begun in New Orleans. I met him in Paris in 1866, and was much gratified to find him in the enjoyment of a fair state of health, but greatly annoyed by the necessity of emptying and cleansing the sac. I measured the latter and found it capable of holding six ounces of fluid, and that the daily secretion amounted to about four ounces. The ribs upon the affected side were considerably depressed, but the respiratory murmur could be heard distinctly in front.

Mr. W. returned to Mobile in 1868, where he still resides. His general health is somewhat feeble, but he is able to attend to all the ordinary duties of life without any special distress. In a recent letter he informs me that the cavity has undergone no change since 1866; that it still measures about six ounces in capacity, and secretes from two to three ounces of pus daily.

Considering that the operation was original in its conception[1] and performance, and resulted in preserving a valuable life, it may seem a little invidious to criticize it, but one cannot *now* shut his eyes to the fact that, if the two or three ribs below the one which was trephined had been subsequently excised, the probability is that complete obliteration of the sac would have occurred.

In connection with the preceding case I take the liberty of

[1] I am quite sure that Professor Stone had not heard of the suggestion of Roser, referred to in the early part of this article, nor the operation of Walter, mentioned in a preceding foot-note.

alluding briefly to another which came under my care a few weeks later.

Case II.—Captain H., of the Confederate States Army, entered the infirmary on or about March 15, 1862, suffering with empyema, resulting from a penetrating gunshot wound received some weeks before. A small fistulous opening existed between the seventh and eighth ribs, an inch or two beyond their cartilages, but it was not competent to keep the pus cavity drained, nor could it be sufficiently dilated for this purpose, in consequence of the near approach of the two adjacent ribs. Acting upon the experience I had already gained, I applied a large trephine to the eighth rib, immediately below the fistula, divided the obstructing membrane, and thus released a large accumulation of fetid pus. The cavity was thoroughly cleansed by a weak solution of carbolic acid, and a plug of roller-bandage inserted into the opening. The patient was sufficiently recovered to leave for his home in Texas within a week or ten days. I heard nothing of or from him for three years, when he presented himself at my office to show me the result and to thank me for the operation. The chest was somewhat contracted, but not sufficient to produce marked deformity; the opening was closed by a firm cicatrix, and the respiratory murmur could be heard within a few inches of this point. He informed me that closure occurred within a few weeks after he reached home. From recent accounts Captain H. is still alive, actively engaged in business, and in the possession of excellent health.

Complete recovery in this case was evidently due to the fact that the disease was situated opposite the most movable portion of the thoracic walls. The falling in of the latter was sufficient to meet the partially expanded lung, and the cavity, kept constantly drained, was soon entirely obliterated.

Since the last-mentioned case, which occurred twenty-one years ago, several of a similar character have entered my wards at the Charity Hospital, and undergone the same treatment, but, owing to the nomadic habits of the patients, it is impossible to say what the ultimate results have been. In every single case, however, up to the time of leaving the hospital, there were good reasons for making favorable prognoses. As one of these cases

presented an exceptional complication, it is worthy of special mention.

M. Barry, æt. 27 years, came under my care in December, 1881. He had been shot from behind, through the right lung, twelve months before, the ball emerging at the eighth intercostal space in front, a little in the rear of the junction of the adjacent ribs with their cartilages. A low form of pleuro-pneumonia followed, accompanied by a free discharge of offensive pus from the two openings, and expectoration of a similar fluid from the lungs. After a protracted illness, during which the right side of the chest became greatly contracted below, the wound of entrance closed entirely, and, in consequence of the approximation of the ribs, the opening of exit was so reduced that only a small quantity of fluid escaped in that way. When I first saw him he was suffering from irritative fever and hectic, coughing up large quantities of fetid pus, and could scarcely bear to be propped up in bed a sufficient length of time to be examined. After several partial efforts, I succeeded in determining the presence of air and pus in the pleural cavity, a tolerably free communication between the latter and the bronchial passages, and the small fistula in the eighth intercostal space.

A few days after the diagnosis was completed, and in the presence of the medical class, I applied a large trephine to the eighth rib, just below the fistula, and then with a bistoury divided an unusually densely thickened pleura, forming the wall of the sac. Immediately air rushed into the cavity with a deep gurgling noise, and the next instant out dashed a spluttering stream of putrid pus, which bespattered every one around, and filled the amphitheatre with its nauseous odor. At the same time, and doubtless in consequence of the entrance of the cool air, a violent cough was excited, accompanied by the expectoration of a shower of the same foul fluid mixed with blood, much to the chagrin of the assistants who were out of range of the opening in the side. It was a most disgusting experience. However, not to be tedious, more than a pint of fluid escaped, and, when the patient's agitation had subsided, the cavity was gently washed by means of a rubber-ball syringe with tepid carbolized water, but little of which entered the bronchial tubes. I need not enter into the details of the subsequent treatment further than to say that the injection was repeated twice a day, the opening being plugged in the mean time with a small roller bandage.

The patient rallied rapidly. In a week all communication between the suppurating cavity and the air-passages was closed, and, in about six weeks from the time of the operation, the cavity was so nearly obliterated that longer detention in the hospital was not deemed necessary. I have no reason to doubt that entire recovery was the final result.

DISCUSSION.

Dr. J. Ewing Mears, of Philadelphia, Pennsylvania.

I have been very much interested in the reports of the cases presented by Dr. Richardson. I wish to call attention to a method of accomplishing thorough drainage, suggested originally, I believe, by the late Dr. Hodge, of Philadelphia, and which I have practised in cases of empyema. I have found no difficulty in securing complete drainage of the suppurating cavity in those in which I have employed the plan. Drainage is effected by the introduction of a large perforated rubber-tube by means of a hernia needle. The needle is introduced into the cavity of the chest above, carried down through the cavity, and brought out below. The hernia needle has a slit in the point through which the tube can be slipped into the eye. The needle, thus armed, is withdrawn and the tube follows. Care should be taken to pass one-half of the length of the tube through the eye, in order that a portion should not be left in the cavity if the traction should happen to divide it. I have passed the needle from the upper border of the sixth to the upper border of the eighth rib. I have introduced the tube in two instances recently: in a child eight years old and in a man of thirty-two, both suffering from chronic empyema. In the boy the tube remained in position for six months. It was then withdrawn and the opening closed in a few days afterwards. This method of introduction, I believe, prevents undue pressure upon the tube by reason of the oblique direction it takes in passing into the cavity between the ribs. In both instances I incised the tissues about the openings so as to afford free movement of the tube. Free drainage was accomplished, and also complete cleansing of the

cavity by the injection of carbolized water. It seems to me that the perforated tube in the cavity assists in collecting and affording escape to the pus. Moreover, the daily change in its position prevents contraction about the openings and maintains direct channels for the exit of the fluids. The obstruction presented to the escape of fluids is due, I think, in many instances, more to the thickened pleural membrane than to the want of space between the ribs.

Dr. T. F. Prewitt, of St. Louis, Missouri.

This case recalls one in my own experience in which the difficulty of obtaining thorough drainage, by reason of contraction of the ribs, is strongly illustrated. A woman came to me a few years ago with an opening in the chest, which had supervened upon an old pleurisy. She had fever and cough. Her general health was impaired, and it was evident that there was not sufficient drainage. I enlarged the opening, and passed a uterine sound in; passed it back into the chest, and by careful manipulation found its point, and made a counter-opening in the eighth or ninth intercostal space. I then passed a tube through, so as to secure better drainage and cleansing. Through the opening there was a large amount of extremely offensive pus discharged. After this, the fever disappeared, the general health improved, the patient gained in flesh, and was doing admirably well. But in a few months she appeared again. I recognized at once, as Dr. Richardson has pointed out, that there was pressure on the tube so as to interfere with the proper drainage. I then resected the rib posteriorly, and continued the washings. She again improved; but there came a sudden change, which I am somewhat at a loss to explain. In washing out the cavity with carbolic acid solution one day, she suddenly complained of pain, then became unconscious and pulseless. She was placed in bed and morphia was administered. I thought at first that she was suffering from shock, but she continued to complain of pain at each dressing. Subsequently, there was evidence of an accumulation of pus below the opening, and whenever I would attempt to manipulate the wound, she would complain of intense pain and difficulty of breathing. I could only infer that ulceration into the mediastinal space had occurred, and that the opening was so large, that, in washing out the cavity, the fluid had passed through, and that suppuration had occurred in the mediastinal space.

Dr. H. F. Campbell, of Augusta, Georgia.

Mention has been made of using a large trephine. It seems to me it would be a difficult instrument to use between the ribs, even on an adult. I should think that a chain saw or a Hey's saw would be better.

Dr. Richardson. In resecting the rib with bone-pliers or the chain saw, the rib after the first division shakes a good deal, and it is very difficult to make the second division, whereas with the trephine you have no such difficulty, and on account of the softness of the rib you can make the opening in a very short time. You do not want a very large opening behind, but in front a pretty large one, because it contracts a good deal. On account of the softness of the ribs they yield freely to the trephine.

I would like to add one thing; that I have recommended the use of the trephine only in cases of empyema associated with injury, and not in the idiopathic form. But I may say that I have performed the operation in one case of the latter variety, and, I am very sorry to say, with a fatal result.

VALUE OF EARLY OPERATIONS IN MORBID GROWTHS.

By S. D. GROSS, M.D., LL.D., D.C.L. Oxon., LL.D. Cantab.

[Read June 1, 1883.]

The value of early surgical interference in morbid growths, especially the malignant, has long been recognized by writers and teachers; but, unless I greatly err, it has not been discussed with that degree of force and emphasis to which its intrinsic merits entitle it. I, therefore, purpose laying the matter before you, if possible, in a stronger, if not also in a clearer light, than any in which it has hitherto been presented, and in this wise bring it more fully under your notice for elaborate and dispassionate consideration.

The great reasons for the removal of tumors in the early stages of their development, may be thus stated: 1. The less risk of shock and of hemorrhage; 2. The more effectual riddance of the diseased structures; 3. The diminished probability of septicæmia, or blood-poisoning; 4. The avoidance of unsightly scars; and, 5. The less risk of a recurrence of the morbid action, either at the seat of the operation or in other parts of the body. These propositions are so self-evident that any formal discussion of them seems to be out of place; nevertheless, it may be well to call special attention to a few points in order to impress them more fully upon your minds.

The fact is now generally, if not universally admitted, that all morbid growths or tumors, whether benign or malignant, are of local origin. That the constitution, in certain conditions, as when from any cause the general health is more or less seriously impaired, may predispose to such formations, is not improbable; but that a neoplasm can be developed in any organ or structure

of the body of a perfectly sound person, without some local cause, is what no enlightened pathologist of the present day believes or teaches. There is unquestionably occasionally a hereditary tendency to the development of morbid growths, both benign and malignant. We see this tendency, sometimes in a remarkable degree, displayed in warts and sebaceous cysts. Examples of the latter I have repeatedly witnessed in three generations, and instances doubtless occur in which this disposition manifests itself still further. Malignant diseases, as carcinoma and certain forms of sarcoma, occasionally betray a similar tendency. As many as three, four, or even five cases of scirrhus of the mammary gland have been noticed in as many different members of the same family. Epithelioma of the lip, skin, vagina, and uterus occasionally exhibits a similar freak.

I state, as another incontrovertible proposition, that all morbid growths, tumors, or neoplasms are the product of perverted nutrition, in which the comparatively few cells native to the part are replaced by colonies of young cells, of the latter of which the new product is essentially composed.

All morbid growths are developed, directly or indirectly, under the influence of inflammatory action, the result of external injury, or, as is more frequently the case, of some mechanical obstruction, causing, in the first instance, congestion of the part, and this, in turn, inciting action and inflammation, both leading sooner or later to abnormal cell-growth, cell-formation, or cell-development. It is in this way, and in this way alone, that we can satisfactorily explain those morbid growths, both benign and malignant, which, as the phrase goes, arise without any assignable cause. One of the most simple of all tumors, the sebaceous, is formed under the irritating influence of its own natural secretion, retained by the closure of its natural outlet. Obstruction of a lacteal duct is, there is no doubt, a frequent starting-point of scirrhus of the mammary gland. There is not a surgeon of any experience anywhere who has not occasionally met with cases of carcinoma which were due, directly or indirectly, to the effects of local injury.

Having laid down these propositions as so many fundamental

principles, we are now in a position to take up the question of the importance of early surgical interference in morbid growths. The remarks which I shall offer under this head are intended to apply more especially to malignant neoplasms.

There are some tumors whose distinguishing features are so well marked, even in their earlier stages, that he who runs may read. On the other hand, great difficulty often presents itself, sufficient to perplex and puzzle the wisest and most experienced head, as well as the most cautious observer. When the tumor is fully developed, when its features stand out, as it were, in full relief, there can rarely be any doubt about its true nature, certainly seldom in the mind of an educated surgeon. Unfortunately, few of the cases of malignant disease, either in our rural districts or even in our larger towns, come under our observation in time for early surgical interference. In the great majority of instances the mischief, in the form of great structural lesion, if not serious constitutional involvement, is unfortunately effected before the scientific and enlightened practitioner has an opportunity of inspecting the morbid product. Three circumstances may be enumerated as contributing mainly to this result: the want of correct diagnosis, the dishonesty or knavery of the professional attendant, and the folly or stupidity of the patient.

It is in vain to conceal from ourselves the fact that there are comparatively few reliable, ready, or trustworthy diagnosticians. Diagnosis is a high art, and it is not saying too much that the profession, as a body, are not sufficiently familiar with it to render it at all times, or even in a minority of cases, properly available at the bedside. It is, of all the arts in our profession, the one which demands the greatest amount of refined culture, experience, and tact, not culture, experience, and tact of an ordinary type, but of the highest possible type. Founded essentially upon a thorough knowledge of pathological anatomy, a branch of science little cultivated in any of our schools, and totally neglected in most, it is not surprising that the art of diagnosis should be so little understood by the generality of practitioners, and so many errors committed in the examination of morbid growths. If there is any one thing in the organization of our

medical colleges more culpable, I had almost said more criminal, than any other, it is the exclusion from their curriculums of the study of pathological anatomy. Just in proportion as our knowledge of morbid structure is positive, accurate, and comprehensive will be the probability that we shall become skilled diagnosticians, and conversely. Hence, so long as this state of things exists, we shall look in vain for any marked improvement in this direction, and what is true in this respect is true alike of city and country practitioners, standing, as they do, upon the same unfortunate platform.

Knavery, dishonesty, and self-conceit are found in all ranks of life, and among all classes of men. That our profession should be wholly exempt from these infirmities is not to be expected. It abounds in charlatanry, in men who promulgate their claims in all kinds of ways to allure and deceive the people by holding out false pretensions of cures, which they well know can never be realized. In this manner important time is often lost by permitting the case to progress until the propitious period for affording relief has passed by. In making these remarks, I desire to apply to the term "charlatan," a wider signification than the one in which it is usually employed. There are "quacks" in every profession, the clerical and the legal not less than in the medical; men who do not hesitate to prostitute their honor to self-interest, and make the merest trade of their vocation. Money is their god, and they worship him in any way best adapted to their object. Such men are not any better than the avowed, advertising charlatan.

A patient laboring under a serious morbid growth may consider himself as peculiarly fortunate if, in the early stage of his troubles, he fall into the hands of an intelligent, upright, conscientious surgeon. Such a man will not hesitate to express his honest convictions. He will not esteem it a disgrace to say to his patient, "Your case is one I do not comprehend; it is an uncommon case; it is a case beyond the grasp of my limited experience; go and consult some one whose opportunities of observation have been more extensive; and be sure to lose no time in carrying my advice into effect." The knave, on the other

hand, will do all he can to mislead his patient, and lull his fears by assuring him that his disease is of no importance, or by telling him that it will either remain stationary, or, in time, disappear spontaneously. He is afraid to tell the truth, lest by showing his ignorance he should lose prestige, and thus damage his practice. To cap the climax of his impudence, he may even assure his patient that he is fully master of the situation, and that he knows as much about the nature of the case as the wisest and most experienced surgeon in the land. Now, it is precisely men of this stamp, and their name is legion, that do the mischief. Ignorant of the art of diagnosis, or incapable of telling the truth, they allow the case to proceed from bad to worse, until the poor patient, in a fit of desperation, seeks, as a *dernier resort*, relief at a more enlightened and more honest shrine. The examination now made reveals the fact that the disease has made hopeless progress, and that any operation, if proper at all, can afford only temporary relief. Let me instance, as an illustration of my meaning, carcinoma of the female breast.

This disease, as is so well known, generally begins in the substance of the mamma as a small nodule or tumor, hard on pressure, and the seat of occasional pain of a shooting or darting nature. The woman is forty-five years of age. As the morbid action advances, the growth enlarges, the pain increases in severity and constancy, and, by and by, retraction of the nipple is noticed. Gradually the growth becomes more and more fixed in its situation, and if now the glands in the axilla—naturally so diminutive as to be scarcely distinguishable—be examined, they will be found more or less enlarged and indurated. The diagnosis is not difficult. The case is one of scirrhus, nothing else. A careful examination of the breast, and a careful consideration of the history of the case, leave no doubt as to its true character. If the patient is under forty years of age, or from twenty-five to forty, with a hard, movable nodule, the seat of occasional darting pain, especially annoying during the menstrual period, unaccompanied by change in the nipple, in the surface of the breast, and in the axillary glands, we assume that the neoplasm is a fibroma or adenoma, and unhesitatingly

assure the patient that excision of the growth will eventuate in a complete cure.

In carcinoma of the breast, the honest and enlightened surgeon does not wait for involvement of the axillary glands or serious structural disease. He knows that the patient's safety, present and prospective, lies in the early use of the knife and in thorough excision. He urges the importance of prompt interference, and assures the patient that, if the operation be properly done, it will probably eventuate in a permanent cure, or, if relapse occur, that there will be a comparatively long exemption from suffering. Everybody knows what the result of excision of the mammary gland in ordinary cases of cancer is; how rarely the disease is completely removed, and how few women live beyond eight, ten, or twelve months after such interference. In all such cases, cancer-cells have invaded the neighboring structures beyond the reach of the knife, especially as ordinarily employed, and serve as foci of new neoplasms. It makes one's very soul ache to see so many women doomed to endure the most frightful suffering in carcinoma of the breast for the want of a correct early diagnosis and the thorough ablation of the diseased structures. Of all the agony I have ever witnessed, there is none at all comparable to that arising from this form of malignant disease of the mammary gland in its more advanced stages.

Finally, there is a class of patients, the subjects of neoplasms, who, influenced by timidity or mock modesty, wilfully deceive themselves. Such persons are fully aware that there is some disease going on somewhere—that there is a thief concealed in some part of the house—but they are afraid to have the matter properly investigated, lest their worst fears should be realized, and they should be thus made miserable. They conceal from themselves, as well as from their friends, their true condition, and when this condition is at length discovered, it is generally found to be too late to afford any benefit, except of the most transient kind, by surgical interference. Like the ostrich, they hide their heads under their wings in the delusive hope of thus

escaping the danger which threatens them. Of such cases, I have seen not a few in the course of my practice.

What has now been said is true alike of carcinoma, of sarcoma, and even of benign growths. In carcinoma, the cells are conveyed from the original disease by the lymphatic vessels, and in sarcoma by the bloodvessels, whereas in the ordinary tumor the increase is due simply to cell-proliferation, which often proceeds with extraordinary rapidity, the growth in a short time acquiring an enormous bulk.

In all operations undertaken for the removal of neoplasms of whatever nature, the golden rule is to perform the work as thoroughly as possible. If this cannot be done it is better in many cases, if indeed not in all, not to meddle with the growth at all, as such interference often only tends to light up increased activity, not only in the abnormal structures themselves, but in the surrounding ones. In carcinoma, the knife, as already repeatedly stated, cannot be employed too early. The longer it is withheld the greater will be the probability, not of relapse, properly so called, but of some of the cancer cells being left behind, buried out of sight, and thus serving as new centres of morbid action. All secondary developments, as enlarged and indurated lymphatic glands, should of course be removed in immediate succession. All sarcomatous neoplasms are, as a rule, dangerous formations, however early subjected to the knife, not so much on account of any inherent tendency to recurrence after extirpation, as from their liability to attack other parts of the body from causes similar to those which gave rise to the primary disease. The worst of all the sarcomas is the round-celled. Any rapidly growing tumor is, as a law, a bad subject for successful surgical interference. In excising the mammary gland for the removal of carcinoma, the plan so successfully adopted of late years by Dr. S. W. Gross, of cutting widely around the diseased structures, and healing the wound by the granulating process, is worthy of all praise. In a number of cases thus treated by him, the result has been eminently gratifying, one of the patients remaining perfectly well after the lapse of nearly five years from the time of the operation.

All benign growths of rapid development cannot be extirpated too soon. Let us, for example, take a cystic tumor of the ovary. No one will deny that a neoplasm of this kind of small bulk can be removed more easily, and with less risk to life, than a large one, or one of long standing. In the former case, there will probably be no adhesions, and little or no danger of hemorrhage or shock; a comparatively small incision will suffice to expose the mass, and the resulting peritonitis will be restricted within the healthy limits, the limits of repair. In ovarian cysts of large size, the reverse of all this is usually the case; there is also apt to be greater shock and greater liability to blood-poisoning. Similar remarks are applicable to chondromas, fibromas, and osteomas; operated upon early in their progress, their removal is generally easily effected with little or no risk to life; allowed to remain until they have attained a large bulk, the troubles and dangers of operative interference are augmented a hundredfold. Nothing more happily illustrates the truth of the old adage, "A stitch in time saves nine," than early operations for the cure of such growths.

What should be the rule of action of the surgeon in cases in which, as for instance, in carcinoma of the breast, in which, perhaps, several operations have already been performed, there is a large ulcerated surface, attended with excessive pain and a large quantity of the foulest discharge, poisoning the very atmosphere which the sufferer is obliged every moment to inhale? Surely no sensible and humane practitioner would hesitate in such a condition to do the only rational thing to be done—to sweep away the whole mass of disease, so far as it is accessible to the knife, and then treat the wound and the system upon general principles. Such a procedure is the only feasible one of relieving pain, of moderating suppuration, and of making the patient temporarily comparatively comfortable. Death under such circumstances is generally hailed as a welcome visitor. In malignant disease of an extremity, attended by such a state of things, the proper remedy would, of course, be amputation without any ulterior hope of a cure. In the throat, portions of the sprouting mass may be clipped off with the scissors, and

in the nose and the vagina, scraped away with special instruments. When all hope is at an end, the only thing to be done is to impart a silver lining to the remnant of life by means of anodynes, administered in quantities sufficient to relieve pain and promote sleep.

What I have here said respecting early surgical interference in malignant and other morbid growths, applies with equal force to many other surgical affections, as well as to diseases in general. Thus an abscess, if not opened early and freely, is sure to cause serious destruction of tissue, to say nothing of the concomitant pain and other distress. A small stone is more easily crushed or extracted than a large one, while the risk to life is incomparably less, inasmuch as there is less danger from shock and hemorrhage, laceration of structure, urinary infiltration, pelvic cellulitis, peritonitis, pyæmia and uræmic poisoning. Besides, the longer a stone remains in the bladder, and the more it increases in size, the greater will be the chance of its causing serious disease of the bladder and of its dependencies.

A pneumonia in its incipiency is, in general, easily managed. Rest in bed, abstinence from food and excitement, and an anodyne diaphoretic, with a turpentine stupe or a dozen dry cups to the chest, usually suffice to cut short the attack in a few hours, or, at most, in a few days. Allowed to progress, it becomes one of the most formidable maladies with which the physician has to cope, and forms one of the great sources of mortality, especially of advanced life.

DISCUSSION.

Dr. Alfred Post, of New York, N. Y.

It would hardly be proper to attempt to add anything to a paper like that we have just heard. But there is one subject in connection with it that I think is worthy the full attention of the Association, and that is the importance of removing morbid growths which seem to be in themselves capable of no harm. Such are warts and other

appendages which are liable to undergo degeneration later. I have seen a mole undergo cancerous degeneration after forty years, and it is my rule to recommend the removal of all such growths as early as possible.

Dr. E. M. Moore, of Rochester, New York.

I have to say that Prof. Gross has uttered what is true, a surgical truism. There is no doubt about the correctness of his assertions. It corresponds so thoroughly with my views that I have really nothing to say.

Dr. E. H. Gregory, of St. Louis, Missouri.

In regard to the early operations for malignant tumors, I think there is no question. In regard to the early operations for all classes of tumors, I also think there is no question. There is a point connected with tumors which strikes me as possessing some special interest. The paper tells us that the earlier a malignant tumor is removed the better. There is no doubt about that; but we know that as carcinoma is developed the local disease becomes less and less capable of transmitting itself to distant structures; that the young malignant cell—if there is such a thing as a malignant cell—is much more readily transmitted from one place to another than the developed cell. Now I can understand that the peculiar cell called the carcinomatous cell is not likely to leave the site where it is developed and go to another place. We do not suppose that these cells produce a fluid which can contaminate distant structures, but we must theoretically suppose that the danger of transmission is much greater in the very inception than at a later time.

Now this is the thought which occurred to me on hearing the paper read. There is nothing in the world so little likely to be transmitted from one seat to another as a developed cancerous structure. When I look at the cells through the microscope, and see their size and awkward shape, and study the matrix in which they are imbedded, I do not see how any transfer can take place. But, on the other hand, I can see how those cells, when they first come into existence, are very liable to change their location. Danger is greatest before the tumor nodule is formed. Infected emigrants are as dangerous as infected immigrants. Then, again, I can understand the difficulty of removing one of these tumors after involvement of

the lymphatics. Of late years, in removing cancerous tumors of the breast, I have been in the habit of going out around the breast, not attempting to save anything, and then going up into the axilla, and removing every part, and actually removing glands not larger than a mustard-seed. But I can conceive that even then there are a great many that I cannot see or feel and take away.

We well know that the so-called sarcomatous tumors are made up of connective-tissue cells imperfectly developed. Then, again, we know that these cells are in contact with the bloodvessels, if not with the lymphatics. We know the bloodvessels are very large and are very readily ruptured, and hemorrhages and aneurismal dilatations of the vessels are common. Whether these tumors are always local or not, is a question I will not discuss. It is very certain that irritation does not produce a tumor except under exceptional circumstances, notwithstanding the assertion of the paper that irritation causes them. There must be some agency to determine the tumor, other than those agencies which determine the inflammatory process. I stated yesterday that the inflammatory process is always secondary to some damage or irritation, and that the inflammatory process is not a morbid process, but a reparative process. I assume that we cannot go behind these tumors any more than we can go behind force and motion in physics; and we know no more to-day about the origin of a tumor than we did in the earliest period of our history. That the agency which is concerned in a reparative process should be the same as that which develops a destructive process, seems to me irreconcilable.

With regard to the early removal of ovarian growths, I am in doubt. It seems to me that opening the abdomen is very dangerous. To read Lawson Tait, it seems as simple as opening an abscess. But it is *not* so simple. I remember a young lady, a school-teacher, who consulted me for an abdominal tumor. I said: "I will not operate upon you; I will let you alone, and I will call to see you every two or three months." Once, about three months after, I called and she was not in. As I was driving away, however, she came running after me, and asked me what I was going to do for her. I said: "As you can run, I will let you alone."

Two years after, however, I could no longer refuse her request, and I operated with success. She has since married. As long as

my patients can be useful, I let them remain useful, and I delay all operative procedure until there is nothing left but to take all the risks and operate. Hence, while I agree with the opinions expressed in the paper in the main, I am doubtful as to the propriety of performing at an early period some of the very dangerous operations.

DISLOCATIONS OF THE ASTRAGALUS.

By BASIL NORRIS, M.D.,
SURGEON U. S. A.

[Read June 1, 1883.]

Owing to the infrequency of dislocation of the astragalus, and the remarkably few instances which have been recorded of its successful reduction, the writer ventures to present a narrative of the injury as it occurred to him personally, accompanied by citations from authors, letters from distinguished surgeons, and reports of cases selected from medical journals.

As described by Sir Astley Cooper, the astragalus is connected above and on each side with the tibia and fibula, below it has

Fig. 1. Fig. 2.

Simple dislocation.

articulating surfaces for its junction with the os calcis, to which it is united by means of a capsular and strong interosseous band of ligament, and anteriorly it is articulated to the os naviculare by a capsular and internal lateral ligament.

Professor Gross best defines the differential diagnosis of dislocation of the ankle-joint and luxations of the astragalus. In the former the astragalus is torn off simply from its connections with the tibia and fibula, in the latter it also loses its relations with the os calcis and scaphoid.

A bony prominence on the dorsum of the foot is the diagnostic sign of dislocation of the astragalus forwards, as it is when under the tendo Achillis the unfailing sign of dislocation backwards.

The dislocation forwards may be complete or partial. When complete it is torn from all its articulations from the tibia and fibula, from the os calcis and scaphoid. When partial the head of the astragalus is separated from the scaphoid and inclines to right or left; or, the anterior ligament of the ankle-joint being ruptured, "the superior articular surface of the astragalus may be felt quite subcutaneous some distance in front of the tibia."

According to Mr. Erichsen, in many cases the so-called complete dislocation is not altogether complete, a portion of the bone still intervening between the under surface of the tibia and the upper surface of the os calcis.

The foot, in partial dislocations, is inclined to the right or left. In its complete form it is turned at right angles to its normal position.

There is reason to believe that partial dislocation of the astragalus sometimes escapes notice. There is hardly a surgeon, we assume, who will not recall a doubtful case of badly sprained ankle, on reading over the cases of partial dislocations of the astragalus to be found among the selected cases herein reported.

An old-fashioned method of reducing a partial dislocation of the astragalus was related to the writer by a German from the Fatherland. When a boy he severely injured his ankle while running across a fallow field. The joint was extremely painful, as he well remembered, and was treated as a sprain. It grew worse and was much swollen, when, by good fortune, a neighbor, and as usual an old woman, advised him to place a brickbat upon the foot, then swinging it back and forth two or three times, to sling the brick as far as possible. By this means the foot was jerked forwards and he was suddenly relieved.

Sir Astley Cooper, in his work on dislocations, says of a simple dislocation of the astragalus, that it is a most serious accident, being very difficult to reduce, and should the reduction not be effected the patient is ever after doomed to a considerable degree of lameness; and Dr. George W. Norris, of Philadelphia, in his *Practical Surgery*, states that the records of our science possess but few cases of luxation of the astragalus not complicated with laceration of the integument. The case of simple dislocation reported in the sixth edition of *Hamilton on Fractures and Dislocations* has hitherto stood "solitary and alone" in the annals of American surgery. He says: "In September, 1870, I saw with Dr. Sayre, in consultation, a dislocation of the astragalus forwards and outwards, in the person of Mr. Stewart, of this city, which had just occurred in consequence of being thrown from a carriage. The dislocation seemed to be nearly complete, causing a great projection and tension of the skin. Under the influence of chloroform, by extension and pressure, it was easily reduced by Dr. Sayre. In five weeks from the time he was able to walk, and soon after the restoration of the functions of the joint was complete."

The case above alluded to as the subject of this paper occurred at seven o'clock on the morning of August 19, 1882.

It happened when driving in my buggy through the Monument Park, in company with a driver, and in consequence of going over an uncovered drain which crosses the road. Coming upon it unexpectedly, the axle of the right fore wheel broke, and the wheel fell off. The horse became frightened, and ran about two hundred yards, when the right hind wheel also fell off, precipitating us both to the ground.

I alighted on my left foot and dislocated the astragalus forwards with a twist outwards.

A party of workmen came to my assistance and placed me in a passing vehicle, by which I was conveyed directly to my home. No time was lost in summoning surgeons, who arrived promptly in carriages which had been dispatched for them to prevent delay. Finding ether and an inhaler ready at hand, they proceeded immediately to put me under its influence, and to reduce

this dangerous dislocation, which was effected in less than one hour after the accident.

It is with much satisfaction that I am enabled to furnish a report, including the method of procedure and incidents of the operation, from each of the surgeons who conducted the manipulations, as follows:—

Dr. N. S. Lincoln reports as follows: On my arrival at your bedside, in company with Dr. Ashford, on the morning of the 19th of August, I found that you had a short time before been thrown from your buggy and had suffered thereby a dislocation of the left astragalus. The foot was turned inwards at right angles with its normal position, and a prominence, perceptibly formed by the displaced astragalus, extended over the cuboid bone, posteriorly beneath the outer ankle. (Fig. 1.) The integument was stretched almost to the point of rupture over the beak-like anterior extremity of the astragalus over the external malleolus, and, to a less degree, over the external process of the astragalus. There was a triple dislocation of the astragalus, above from the tibia, and beneath and anteriorly from the os calcis and scaphoid.

Falling with great violence upon the outer edge of the left foot, that member was bent inwards, and the astragalus was turned at the astragalo-tibial articulation until the articulating surface of the tibia rested on its inner surface, while the external malleolus lay across the trochlear articulating surface; at the same time the astragalus was forced laterally outwards and forwards so that its head rested upon the cuboid bone, while the body projected beyond the outer border of the os calcis beneath the external malleolus. The internal malleolus was so deeply buried in the inner side of the ankle, in the space from which the astragalus had been displaced, as to give rise, at first, to the belief that the malleolus had been fractured. (Fig. 2.)

No attempt at reduction was made until you were believed to be completely under the influence of ether, which was at once administered by Dr. Radcliffe.

Grasping the heel and foot, while Dr. Ashford made counter-extension, I drew the foot strongly downward, and attempted external rotation without effect.

The parts were absolutely immovable by any force at our command. While continuing our efforts, I perceived evidence of

returning consciousness and muscular rigidity, and at once desisted until complete relaxation was effected by further etherization, when a renewal of the manipulations was more fortunate, the tibia resuming its normal position with an audible snap, while the astragalus was forced back into its bed on the os calcis.

The head of the astragalus, however, still remained resting on the cuboid, or rather, partly on that bone, and partly on the external cuneiform. Rather forcible extension of the foot, with pressure upon the neck of the astragalus, readily carried the head of that bone backward and downward into the concavity of the scaphoid, and to our great happiness the reduction of this formidable dislocation was complete.

In my opinion our success was due, first, to the promptness with which the attempt was made, and, secondly, to complete etherization.

Dr. F. A. Ashford adds the following report: When Dr. Lincoln and myself arrived, we found the left foot dislocated inward and the astragalus luxated forward and slightly outward. The sole of the foot looked directly inward with its inner margin upward and the whole foot somewhat extended. On account of the extreme inward rotation it was supposed from ocular inspection that the internal malleolus had been fractured, but, upon manual examination, this was found to be intact.

Sulphuric ether was administered, and when narcosis resulted, Dr. Lincoln grasped the foot with one hand back of the heel and placed the other over the instep and made extension, while I steadied the leg, making such counter-extension as was necessary.

The first effort lasted a minute or two, and then the extension and counter-extension were intermitted, inasmuch as the skin, which was so tightly drawn over the external malleolus and the projecting head of the displaced astragalus, seemed about to tear. For a few minutes the skin was pressed and smoothed in opposite directions towards the place of greatest tension, and then the extension was resumed by Dr. Lincoln, while with one hand I pressed the skin from above downward toward the ankle and with the other assisted in rotating the foot outward.

As soon as the foot began to move, an outward twist by Dr. Lincoln sent it into its proper position, with the astragalus partly, but not entirely, reduced. This was, however, easily restored to its place by direct pressure. No laceration of the integument occurred.

Being obliged to leave the city for a week, I was greatly pleased and somewhat surprised, on my return, to find you moving the joint in all directions without pain, except when carried to extremes. There was then very little swelling, and no abnormal heat about the ankle. There was a spot of ecchymosed integument about three-quarters of an inch in diameter over the point where the tension had been greatest, viz., the head of the astragalus, and which attested how narrowly you had escaped sloughing at this point with its attendant evils. Of course there must have been rupture of the three external ligaments, as well as of all ligaments attached to the astragalus, for the amount of displacement seemed incompatible with their integrity.

On the whole, I think you can be congratulated upon an exceptionally good result.

Desiring to obtain information with regard to injuries of this character, I addressed communications upon the subject to a number of surgeons, and received the following replies:—

Dr. S. D. Gross, Philadelphia, Pennsylvania: I have not seen such a case as you refer to in your note of the 1st instant. The accident is an exceedingly rare one, and I warmly congratulate you upon your fortunate recovery.

Dr. Lewis R. Sayre, New York, N. Y.: The case of William Stewart, referred to in Hamilton's Work (page 249), is the only case of the kind that has occurred in my practice.

Dr. Edward P. Vollum, United States Army: I have had two cases of dislocation of the astragalus, in both of which I was compelled to extract the bone in pieces.

The bones were dislocated forward and inward upon the side of the instep, one was broken at the neck, the other remained entire. The integument in both was unbroken, but greatly stretched.

One of them happened in a gentleman who was thrown from his buggy in Jefferson County, New York, the other in a carpenter, who fell from a scaffolding in Utah; both recovered with a fair use of the ankle-joint and foot.

One of them was seen about an hour after the accident, and great efforts were made to replace it, under ether, without success, with

the assistance of a surgeon. The other was seen some twelve hours after the accident. Treatment and result the same.

Dr. J. W. S. Gouley, New York, N. Y.: Your case is very interesting, and of extreme rarity. In twenty-five years of large hospital experience I have never seen a single case of dislocation of the astragalus uncomplicated with fracture of the leg or laceration of the soft parts around the ankle. In fact, I can cite but three cases of luxation of the astragalus. The first case was that of a man who fell not less than fifteen feet below the place he was standing in an unfinished building. He landed upon one foot, the right I think, and the result was fracture of the external malleolus and dislocation of the astragalus forward, with rotation of the bone and much injury to the soft parts, which perished in a few days. I excised the astragalus, which could at no time have been reduced. The patient had a very long convalescence, but eventually made an excellent recovery. I saw him several years after, and he was able to walk as well as if nothing had happened.

In the second case I think reduction was successful, though there was laceration of the soft parts with fracture of both malleoli. This patient also made a good recovery after a prolonged convalescence, and is able to walk well. This second case occurred last year.

The third case is still under treatment in my wards. I did not see him when first admitted. My attention was called to his case only three weeks ago. When received into the hospital several months since, he was found to have sustained a fracture of the thigh and of both bones of the leg low down, and a dislocation of the astragalus, all on the same side. These fractures are now all united, but the astragalus is still out of place, and the patient can walk tolerably well with some varus.

Dr. D. Hayes Agnew, Philadelphia, Pennsylvania: In regard to luxations of the astragalus we have had three at the Pennsylvania Hospital, if I remember correctly, within the last three years. I have seen two private cases, both of a very remarkable character, in which the bone, as well as could be made out, though not separate from the os calcis, was turned on its axis in such a manner that the lateral surfaces were placed vertically, that is, one presenting to the tibia and the other to the os calcis. These were both irreducible. The others which I mention were all reduced,

though I think in one of the three cases the reduction was not complete.

Dr. J. W. S. Gouley, New York, N. Y.: I have just this moment returned from Bellevue Hospital, where I found a case awaiting me, the brief recital of which will probably interest you.

The patient is a young German, who "fell from a ladder" upon his left foot, and sustained a dislocation of the astragalus forward and outward without fracture, as far as I have been able to ascertain, but the diagnosis of luxation was clear and unmistakable, and was verified by my *interne*, and was made clear afterwards by the reduction which I effected after putting the patient under ether. I at once bandaged the foot and ankle, and ordered the application of an evaporating lotion over the bandage.

Before I arrived a cast had already been taken, which shows admirably the nature of the deformity. In a few days I shall send you by express one of the models which I shall have multiplied, and shall request that you present it for me to the Army Medical Museum.

The reason why I have been able to get this cast is, that my *interne* is very much interested in your case. I read him your letter, and he has been on the lookout for such a case ever since, and it turned up sooner than he or I had expected.

Please let me know how you are walking, and how much inflammation ensued from your injury.

In reply I wrote to Dr. Gouley as follows:—

Your interesting letter has just come to hand. It is, indeed, remarkable that you should have found at this time one of these rare cases—a simple dislocation of the astragalus.

My own case was attended with a pretty high grade of inflammation for about forty-eight hours, and my surgeons were fearful of erysipelas and sloughing—so they said—but I did not think of such results, for I was elated and happy over the reduction. A side splint and roller bandage put on Saturday at eight A. M. were insufferable at four A. M. on Sunday. I had them cut off, and allowed my foot to lie on a pillow, with a cold-water dressing. During the second day the pain increased, and at night was at the highest point; in addition, I had a good deal of muscular pain all over me, but greatest in the back and loins.

Being driven to do something more, I took fifteen grains of quinine on my own account, and Dr. Lincoln prescribed twenty grains of salicylate of soda, both these doses were in full force about an hour afterwards, and from their combined effect I was thrown into a profuse perspiration and completely relieved of pain. The inflammation subsided by resolution, hastened, I have no doubt, by the medicine administered.

I then determined to try to get on my side, and for that purpose placed a thick fold of cotton-wadding along the inner side of the leg from the knee to the sole of the foot, and over that laid a straight splint, extending three or four inches beyond the foot. On the outer side I had a carved splint for the leg and foot; it was selected from the supply of splints on hand, and was also padded with cotton-wadding. Both of these splints were fastened on with strips of bandage around the leg at intervals, thus dispensing with the roller bandage and securing an open support for the ankle. With this appliance I was able to turn over on my side without having my foot rub against anything, and I could move it about and rest it on the other leg, protected by the projecting end of the inside splint against the danger of striking the foot by extending the leg, awake or asleep.

On the third day I could move my ankle-joint a little, and did so, against the advice of my friends, who enjoinéd absolute rest.

On the fourth day I directed Mr. Marshall, our hospital steward, to bring roller bandages and paste-board, and I superintended the application of a starch bandage as follows: A roller was neatly applied from the toes to a point above the calf, then the whole bandage, leg and foot, was covered with good flour paste well rubbed in; next another roller and then more paste were applied; over this, on each side of the leg, a side splint of binder's-board, well starched and cut narrow at the end, so as to bend easily and overlap under the sole of the foot, was placed in position; a third roller starched thoroughly was put on, and the dressing finished with a dry roller. The dressing was sufficiently dry in about twelve hours to admit of being slit up along the median line, from the toes to the knee, and along the sole of the foot to the heel. A little gentle and dextrous manipulation enabled my attendant to open the splint and to spring it widely enough open to take it off, so that the leg could be exposed to the air, and rubbed to allay the urgent itching, which already called for the splint's removal.

This movable splint could be put on and taken off at will, and should not be confined with a roller, but with strips of bandage tied like strings.

With this most satisfactory apparatus I was able to move about my room on crutches in one week; all about the house in two weeks, and, at the end of three weeks, out of doors. One month after the accident I was able to take off the starched bandage, and lay aside my crutches for a close-fitting stocking and a gaiter shoe.

Dr. J. H. Brinton, of Philadelphia, reported a case of dislocation of the astragalus in full in the *Photographic Review*, No. 2, December, 1870, as follows: "On the morning of October 20, 1868, Mr. L., of South Carolina, aged 75, fell through a trap-door into a cellar about nine feet deep, striking the rounds of a ladder in his fall. He was unable to rise, and was carried up the ladder on the back of a man. A neighboring physician was called in, a dislocation of the ankle pronounced, and reduction under ether attempted. This failing, the limb was temporarily dressed, and the patient was sent in a carriage to his residence in the country, some ten miles distant.

"At ten P. M., of the same day, I saw Mr. L., in consultation with Dr. Moss, and recognized a complete luxation of the astragalus forwards and inwards. The integument was untorn, but was tensely stretched over the head of this bone and over the inner malleolus, which latter appeared very prominent from the displacement of the astragalus. The foot below the scaphoid and cuboid bones was markedly everted. The fibula was fractured transversely about one inch above the external malleolus. The patient was again etherized, and powerful attempts made to reduce the luxation, but so tightly locked were the bones that no change whatever was effected in the position of the parts. Operative interference being at this time absolutely contra-indicated by Mr. L.'s condition, the limb was placed in a bran-box, and stimulant and opiate remedies administered.

"On the following morning inflammation of an erysipelatous character set in, extending up to the knee. The dorsum of the foot was greatly swollen, and of a dark purple color, but the pulsation of the dorsalis pedis artery remained good. This local condition continued for several days, during which time the patient remained tolerably free from pain and fever, with a good appetite and in

good spirits. By the twelfth day after the accident the inflammatory condition of the leg had in great part disappeared; but at this time the constitutional symptoms became threatening, and a slough commenced to form over the head of the astragalus.

"On the fourteenth day, in consultation with Drs. Moss and Lynah, I excised the astragalus by a longitudinal incision. The removal of the bone was accomplished without difficulty, its ligamentous connections having been in great part severed by the violence of the accident. The tendons of the tibialis posticus and flexor longus pollicis muscles were ruptured and in a sloughing condition.

"The patient after the operation did remarkably well, thanks to the unwearied solicitude of his attending physician, Dr. Moss, his own unflinching courage and determination, and the care with which he was nursed. The limb was placed in a swinging bran-box, the open joint was largely injected with solutions of permanganate of potash and of carbolic acid, and the free exit of all discharges was carefully provided for. At the expiration of twenty-five days from the operation all of the exposed cartilages had softened and had been taken away, and healthy granulating surfaces were established. A little later the superficies of the posterior part of the os calcis became carious and was removed. During February, 1869, the discharge from the ankle-joint gradually diminished in quantity; at the same time rapid cicatrization took place. A splint formed of bandages saturated with a solution of silicate of soda was now applied, but so great was the congestion of the limb when bandaged, or when placed in the dependent position, that the splint could not be tolerated, and was therefore dispensed with. This tendency towards swelling and discoloration decreased with time, cold applications, and friction, but no sustaining apparatus to the limb was ever used.

"In April, 1869, Mr. L. began to walk on crutches, but these were soon exchanged for a walking-stick. At present (November, 1870) the wound is solidly closed, the heel is elevated about one inch and three-fourths, and the foot presents the peculiar double curve exhibited in the photograph. A slight degree of motion exists at the ankle-joint, and the extremity, although shortened, is firm and capable of sustaining the weight of the body without pain. He now walks comfortably with the aid of his cane alone, and wearing a high-heeled shoe without metallic supports. As evidence

of the power yet remaining in the ankle, I may state that, unassisted and without either crutch or stick, Mr. L. mounted to the third story of a building for the purpose of having a photograph taken of the foot."

"In examining the recorded cases of complete simple luxation of the astragalus, I have been struck with the different opinions entertained as to the proper treatment of the accident.

"Most surgeons agree as to the propriety of an attempt at reduction, although from this opinion Nélaton dissents, on the ground that in this injury the attachments of the astragalus are destroyed to such an extent as to involve the life of the bone. Dr. Gross advises immediate excision; Dr. Norris, primary excision, if reduction be impracticable. Mr. Erichsen recommends the attempt at reduction, accompanied, if necessary, by section of the tendo Achillis; if reduction be impossible, then a secondary excision, which, in his opinion, is safer than the primary operation. Streubel, who has laboriously examined the literature of this injury, favors first an attempt at reduction; if that be unsuccessful, then an expectant treatment, to be followed, if necessary, by secondary excision. Such, too, are the conclusions of Turner and of Broca.

"In the judgment of the writer all attempts at reduction should be made with as little violence as possible, and should be of short duration; a secondary resection, moreover, would seem to be less dangerous than a primary one, and should be preferred. The following statistics are pertinent to this subject: Streubel reports fifty-seven cases of primary excision of astragalus, with forty-one recoveries, and twenty-five secondary excisions, with one death. Heyfelder gives sixty-seven exsections, with nine deaths. Mr. Hancock collects twelve cases of excision performed by British surgeons for complete simple dislocation of astragalus, with nine good recoveries and three deaths; and twenty cases by foreign surgeons with fourteen recoveries, three deaths, and three doubtful results. The greater rate of mortality in all the statistics accompanies the primary excisions."

Two instructive cases of reduction of partial dislocation of

the astragalus forwards, of long standing, are reported by Mr. George Brown—read before the Clinical Society of London, February 11, 1876—as follows:—

"Dislocations of the astragalus are, I believe, among the rarer forms of surgical accidents, and are generally most difficult to treat. I have had an opportunity of seeing only two cases of this kind of injury; one the case which I wish to bring before your notice this evening, the other that of a girl about twelve years of age, who was an in-patient of Charing Cross Hospital, when I was house-surgeon to that institution two and a half years ago. In this latter case the anterior ligament of the ankle-joint was ruptured, the astragalus was dislocated forwards, and its superior articular surface could be felt quite subcutaneous some distance in front of the tibia. The patient did not apply to the hospital for some days after the receipt of the injury, being under the impression that it was only a bad sprain. There was but little difficulty in reducing the dislocation, but at first it was almost impossible to keep the bone in position, as it was found that whenever the bandages were re-adjusted the bone had again slipped forwards. However, we were, after a time, more successful in fixing the bone, and at the end of eleven weeks the patient was discharged from the hospital cured, but she found it necessary to wear a special boot with iron supports for six months afterwards.

"The following is the history of the case I present this evening:—

"The patient, Clara P., æt. 16, accidentally slipped down a step about two weeks before Easter, 1874, and twisted her left foot. Previous to this she had nothing the matter with either of her feet or ankle-joints, except that she was naturally flat-footed. The foot was very painful after the accident, but, as it was thought to be nothing more than a sprain, no medical advice was sought. On Easter Monday she took a long walk, after which she suffered great pain of the foot, and there was, the mother states, a decided swelling on its inner side. Next day a surgeon was consulted, who said that there was nothing the matter with the foot beyond weakness of the ankle-joint, due to constitutional causes, and that its mis-shapen condition was a consequence of her being flat-footed. He prescribed rest and strengthening medicines, and said that little improvement could be expected for some time. It should be stated that the gentlemen consulted did not examine the sound foot or

compare it with the other. Several months elapsed, but the foot gradually grew worse, the deformity became more marked, and the difficulty of walking greater. Towards the end of the year she was scarcely able to walk without assistance. Generally she leant on another person's arm when she walked out, but she spent the greater portion of her time either sitting or lying down, as the foot pained her very much after standing or walking. Often the pain was so great that she was unable to sleep at night.

"Early in January, 1875, the mother requested me to examine her daughter's foot and advise as to treatment, as she feared that she would become a permanent cripple.

"On comparing the two feet it was at once apparent that there was displacement of the astragalus forwards and inwards. The hollow which should exist between the internal malleolus and the scaphoid tubercle was entirely obliterated. This was due to the head of the astragalus slipping forwards and inwards, which at this point was much more prominent than either of the two bony processes before mentioned. Over this projection the skin was tightly stretched. In front of the ankle-joint and on its outer side, where the head of the astragalus should be felt, there was a considerable depression. The tendon of the extensor proprius pollicis and long extensor tendons of the toes were much contracted, and the outer side of the foot was drawn upwards. On asking the patient to walk across the room I found that she was unable to place the outer side of the foot to the ground, and that the whole weight of the body, in walking, was borne on its inner side. There was almost entire loss of power of extension and flexion of the foot.

"When I had given my opinion as to the nature of the case, the mother thanked me, and said that she would at once take her daughter to Mr. ——, mentioning the name of a 'bone-setter,' and appeared to be surprised when I told her that the treatment of such cases came within the province of a surgeon. However, I obtained her consent to do whatever I thought necessary.

"Having placed the patient in the supine position, I requested the mother to hold the leg firmly whilst I attempted to restore the bone to its proper position. I then grasped the foot near the toes with the right hand, and forcibly extended it as much as possible, at the same time pressing with the left hand on the projecting head of the astragalus. In a minute the bone slipped into its natural position with a distinct snap, and its head could be both seen and

felt projecting on the outer side, quite filling up the depression which previously existed. The two feet were now, to all appearances, perfectly symmetrical. Having no splint at hand, I was unable to do more than apply a pad and bandage around the foot. On calling the next day (January 10), I found that the astragalus had again slipped out of position during the night, and the foot presented the same appearance as before. This time reduction was effected very rapidly. I then applied a well-padded side-splint with foot-piece to the inner side of the leg, and kept the patient at rest. At the end of eleven weeks I dispensed with the splint, and permitted the patient to take a little exercise. For some time she could not walk without assistance, but she can now—just thirteen months after reduction was effected—walk long distances without giving rise to pain. She has the perfect use of the joint, and the feet are as nearly as possible symmetrical.

"The exact nature of the case is, I think, pretty clear from its history. There is no doubt that when the patient twisted her foot, a little before Easter, 1874, some fibres of the internal lateral ligament of the ankle-joint as well as those of the calcaneo-scaphoid ligament, which so largely assists in keeping the head of the astragalus in position, were ruptured. At first the displacement was very slight, but having lost its chief internal support, the head of the bone was gradually forced forwards and inwards by the weight of the body until the deformity became very marked, and the ankle-joint almost useless.

"In the early history of the case it would have required very careful examination and comparison with the sound foot in order to detect the exact nature of the injury, but when I saw her nine months after the original injury, its true character was obvious at a glance."

A successful case is reported as having been reduced by a new method, by M. Th. Anger, in the *Bulletin et Memoires de la Société de Chirurgie de Paris*, October 6, 1874, as follows:—

"After having ascertained that the case was one of complete luxation, forwards and outwards, a luxation often irreducible, I did not think it worth while to renew the attempts made by the *internes*, and decided to try my method. The patient being stretched out horizontally in his bed, I passed between his thighs a sheet folded in the form of a cravat, and tied the ends firmly to the cross-bars at

the head of the bed as the means of counter-extension. In order to fix my extending bands of rubber on the foot, I cut long strips of cere-cloth (saradrap), which were applied by figure-of-eight turns over the heel and ball of the foot, all turns crossing under the sole of the foot. At the crossing of the turns under the arch of the foot I fixed strong bands of rubber, and tied them to a rod at the foot of the bed. The tension of these bands was not determined, but was estimated to approximate a force of 15 or 20 kilograms. Under the effort of this moderate traction the foot, which was strongly adducted, yielded gradually. At the end of from ten to twelve minutes perceiving that muscular resistance was exhausted, I placed myself on the outer side of the member, then seizing with one hand the inferior extremity of the leg, and with the other the sole of the foot, I put my knee against the astragalus, which, under a moderate force, returned suddenly into its place with an audible snap."

A case of reduction of a dislocation of the astragalus forwards and inwards, of five months' standing, is reported in the *Australian Medical Journal* of 1879, by T. N. FITZGERALD:—

"On the 30th of September, 1878, the patient having been brought under the influence of chloroform, the tendo Achillis was first cut, then finding the tendon of the tibialis anticus tense, it was also divided. A long tenotome was now obliquely slid from about an inch in front of and below the internal malleolus into the ankle-joint, all the attachments between the astragalus and tibia were freely divided, including some unbroken fibres of its annular ligament. The knife was now carefully withdrawn and re-inserted just over the front of the internal cuneiform bone, and pushed obliquely backwards, and made to divide freely all the attachments between the scaphoid and bulging head of the astragalus, the blade being then carefully withdrawn so as not to allow the entrance of the air into the large cavity thus formed. Extension was now made with the foot in the extremely extended position and firm pressure backwards on the head of the astragalus. Suddenly the foot forcibly flexed on the leg, and we had the satisfaction of finding the astragalus slip into its normal position. The limb was carefully bandaged to the knee.

"On the 18th of January plaster-of-Paris bandages were applied from toes to knee.

"On the 4th of February the patient walked pretty well on the injured foot.

"March 26th, movements of ankle-joint pretty free, could walk moderately well."

Dr. JAMES A. GRANT records in the *Canada Medical Journal*, October, 1865, the following case: "J. M., aged 35, a robust farmer, while driving a cart, June 24, 1864, in consequence of his horse taking fright, was thrown out with violence and dragged for some distance, his foot catching in the wheel as he fell. He attempted to walk, but was unable, observing that he trod on the outside of the foot, at the same time the suffering was very great. The boot being removed, the following appearances were to be observed: The foot was inverted so that the sole looked inwards; the tendo Achillis was not tense; the astragalus was driven forwards entirely out of its place, and assumed a transverse position, the anterior extremity protruding fully an inch through the integument on the outer side of the foot. On the upper part of the tarsus the skin was stretched tightly over the displaced astragalus. There was no fracture of either the tibia or fibula. There was very little surrounding effusion, so that the outline of the various parts could be well defined. The dislocated bone was thus wrenched from all of its connections, and thrown transversely across the tarsus. An unsuccessful attempt was made to reduce the dislocation, by extension and pressure. The limb was placed quietly on the bed, and by moderate pressure the bone turned and shot into its place quite unexpectedly. An outside splint was forthwith placed on the leg, and a cold lotion applied over the ankle. An opiate was given at bed-time, and the following day he entered the General Protestant Hospital. The cold-water cloths were constantly applied until the eighth day after the accident, and then a purulent discharge from the joint having been observed, a linseed poultice was substituted. The discharge continued, more or less, until the end of August, at which time the wound closed perfectly. The limb continued in a weak state until March, 1865; he was then able to throw aside supports and use the limb with considerable freedom. From this date the parts gained strength, and at present he walks about as well as ever, and performs the various duties of farm life, the limb having regained perfect motion; the parts surrounding the displaced bone resembling in every respect those of a healthy joint."

DISCUSSION.

Dr. R. B. Bontecou, of Troy, New York.

It was my misfortune, about thirty-five years ago, to meet a case of this nature, soon after beginning practice. The dislocation occurred in a strong laboring man, and it was downward and outward in character. I obtained assistance, but could not reduce the dislocation. I had not the courage then to do what I would do now. I would now lay the tissues open on the outer side, reduce the parts, and replace the soft tissues. This man, however, recovered so that he has been employed as a coachman ever since. There was no necrosis of the bone, or sloughing of the soft tissues, and the limb has been useful ever since.

Dr. E. H. Gregory, of St. Louis, Missouri.

I have seen but one case of dislocation of the astragalus. It was in a female. The bone was dislocated forwards and downwards. It was the result of a carriage accident. The only remarkable thing about it was the ease with which it was reduced.

Dr. J. R. Weist, of Richmond, Indiana.

I have seen one case of dislocation of the astragalus, in a soldier applying for a pension, in which the dislocation was not very marked.

Dr. E. M Moore, of Rochester, New York.

I had a case a few years ago, very much like that reported. I was convinced that the views of surgeons were not very definite in regard to the treatment of this injury. It has been taught that we should exsect the bone in cases of dislocation of the astragalus, because the operation would be required at some future time. Now I think that there are cases in which this is never to be done, and yet other cases in which it should be done. I had a case in which there was great difficulty in effecting reduction. The patient was etherized, and pulleys were used. We succeeded at last. The inflammation was not great, and the patient did very well. In cases of fracture accompanying the dislocation, it is quite different. I have seen two cases where the astragalus was split into two parts, one part being dislocated and coming down under the tendo

Achillis. One case was that of a woman who was injured by a fall from a horse. I was determined that it should be reduced. Extension and counter-extension with manipulation of the parts were practised, and great efforts were made to accomplish reduction. At last reduction was effected. I decided to apply a plaster bandage to hold the bone in place. While I was preparing the bandage dislocation recurred. I could now reduce it easily, but its retention could not be accomplished. Suppuration did not occur, but lameness supervened.

I was afterwards called to see a man who had met with almost precisely the same accident, and in that case I made no attempt at reduction.

Dr. W. W. Dawson, of Cincinnati, Ohio.

I must say that I have had a limited and rather unpleasant experience with dislocations of the astragalus—limited to two cases—one compound, and the other simple and complete. In the latter the bone was thrust forward, and I found its reduction impossible. It seemed to be fixed, immovable. I can understand how, in cases where great damage is done to the ligaments, that the reduction would be comparatively easy; such was the condition, doubtless, in the case reported by Dr. Gregory. It may be long before he encounters another refractory astragalus so easily managed. In my case the tendo Achillis was tense and cord-like; but I could not prevail on the friends to consent to its division. I felt then as I do now, that its division would have released the displaced bone. This patient, by having a shoe fixed so as to protect the bone, was able to walk, but very imperfectly.

Dr. Moses Gunn, of Chicago, Illinois.

In a somewhat ample experience, extending over the larger part of a century, it has not been my fortune—or misfortune—to meet with a complete dislocation of the astragalus. I met, however, in the last year a complicated case which bears a little upon two points that have been made, and, perhaps, may therefore not be without interest. It was a secondary case. That is, it had existed some three months, and required excision. I resorted at once to excision of the dislocated head of the astragalus. It was a complete dislocation of the head of the bone outward. There was no necrosis. There was sufficient vascular connection to prevent the occurrence

of necrosis. That was a point brought forward in the paper as indicating the necessity of immediate exsection. Here a dislocated bone had for three months retained its vitality. It refers also to the question of the necessity for primary or secondary excision. I am satisfied that had primary excision been made in this case, reduction could have been accomplished. But, as it was, it required a good deal of pressure, continued for several weeks, before the form of the foot could be restored. It was, however, a good foot afterward for purposes of locomotion.

In connection with this subject, I desire to mention another case, which was of an unusual character. A number of years ago, on one of my visits to the country, I saw lying on a physician's table a part of an astragalus. It was evident, from its being still stained by blood, that it was the result of a recent accident. The fragment consisted of the body; all of the astragalus, except its head. There had been a fracture through the neck; the head of the bone was not present. The accident, it was stated, was the result of a fall upon the foot, and the surgeon had simply removed the fragment with his fingers. The astragalus was fractured at the neck, and the body had slipped out. The account that the surgeon gave was that he placed it in as good a position as he could, and the patient made a fair recovery. The case was an interesting one.

Dr. Moore. It strikes me that the interesting point in these cases is how to treat those in which reduction cannot be effected; whether the bone is to be removed at once or subsequently. It has been stated that good authority says remove the bone at once. Good authority also says *wait*. I believe Dr. Gross advises immediate removal. I believe it is better to remove the bone early than to wait. If we remove it early, the question arises whether it would not be well to extend the operation sufficiently far to enable us to see the relations of the parts and replace the bone; then to treat the case as one of compound injury.

Dr. Norris. I desire to call attention to the fact that the last case reported in the paper is that reported by Dr. Grant, of Canada, a case which goes to show that simple manipulation is more important than forced extension. In several cases of compound dislocation which resisted all efforts, the foot was placed on a pillow, and gentle manipulation with the fingers resulted in an unexpected reduction of the dislocation.

EXCISIONS OF THE TARSUS,

With a Report of Two Successful Removals of the Entire Tarsus.

By P. S. CONNER, M.D.,
PROFESSOR OF ANATOMY AND CLINICAL SURGERY IN THE MEDICAL COLLEGE OF OHIO, ETC.

[Read June 1, 1883.]

Admirably adapted as is the foot to sustain weight, diffuse force, and secure ease and quickness of movement, it is, in its proximal half at least, peculiarly liable to disease and the extension of inflammation from part to part. Placed where it must, of necessity, be subjected to violent jars and severe twists, every opportunity is afforded for the occurrence of limited blood-extravasations in its bones, and of traumatic synovitis in its joints.

Its skeleton is made up almost entirely of cancellous tissue, covered in by closely adherent periosteum, that blends so intimately with the ligaments of the numerous articulations that it may, surgically considered, be regarded as a single sheet inclosing the whole tarsus.

It is crossed by numerous tendons and overlain by a definite though not very thick sheet of connective tissue, so that thecal and fascial inflammations may readily, by contiguity, be carried over to it.

In those who, by possession of the *ignotum quid*, are predisposed to the so-called strumous affections, the frequently occurring, usually slight, traumatisms may readily be followed by simple inflammation, or by the deposit of tubercle in the bones, the synovial membranes, or the peri-articular structures.

Specific periostitis or gumma may be here located, to produce the same effects as elsewhere. Lastly, external violence, contu-

sions, cuts, compound fractures (gunshot or other), may be the developing cause of caries or necrosis.

Little wonder is it then, that tarsal disease is of frequent occurrence, or that it is at times found to affect a large portion or the whole of this section of the foot.

When thus extensive, what shall be done for its relief? Rest, compression, and stimulating or sedative applications (always proper in the beginning, and at times securing the best of results) are very likely to fail in arresting the progress of the disease, which more or less rapidly goes on to the destruction of the part.

When such has been produced, when the peri-articular structures are greatly thickened, abscesses have formed, and numerous sinuses exist leading down to dead bone, surgical interference is limited to: 1. The opening of the abscess cavity, and the informal removal of carious or necrosed tissue, with or without deep cauterizations or such local stimulant applications as shall tend to secure more healthy and reparative action; 2. Amputation, usually at or above the ankle-joint; and, 3. The methodical excision of such and so many of the tarsal bones as are unhealthy.

The first method in one form or another has long been employed, and in the less severe cases may be expected to often, perhaps generally, result favorably.

It is under this head that we must place the use of the actual cautery, so highly commended by Ollier and others of the French surgeons; the *évidement* of Sédillot; and the gouging and oakum-seton method of our distinguished Fellow, Dr. Sayre.

By surgeons generally, amputation through the leg or at the ankle has been and still is regarded as much to be preferred to any more conservative treatment.

Such operation, it is claimed, is less likely to be followed by death from septic infection, or as the result of protracted and profuse suppuration; and leaves the patient in a better local condition, more able by the aid of an artificial limb to move about and earn a livelihood.

But there have been reported from time to time, especially

during the last twenty years, cases of extensive formal excisions, the history and end-results of which compel a reconsideration of the alleged dangers and disadvantages attending an attempt to remove the diseased and preserve the healthy part of the foot.

Such attempt, it has been held, may cause death, at first or later; will very likely be followed by a return of the disease in the parts left; and even if successful as respects life, must leave a useless foot, if not one actually an incumbrance.

Before proceeding to determine, if possible, the correctness or incorrectness of these views by an analysis of the 108 excisions of two or more bones which I have been able to collect, permit me to briefly report two cases in each of which removal of the entire tarsus was recovered from, and a very serviceable foot secured.

Case I.—A. S., Alsatian, æt. 39. First seen July 18, 1875. For about two years had had disease of the right tarsus, following, as claimed, a slight injury upon the outer side of the foot.

General condition bad, consequent upon suffering and profuse suppuration and a very recent attack of erysipelas extending up to the middle of the thigh. The posterior half of the foot was much enlarged, and through four openings over the ankle and heel the probe could be readily passed into carious bone.

Excision being determined upon, on the following day, July 19, 1875, the entire tarsus, every bone of which was more or less diseased, was removed through an external lateral incision, extending back to the outer border of the tendo Achillis. The upper articular surface of the astragalus, though healthy, was taken away with the rest of the bone, but the malleoli were left untouched. The resulting cavity was lightly packed with carbolized oakum, and the limb placed in a fracture-box.

A high temperature (105° F.) was noted at the end of the first twenty-four hours; but thirty-six hours later, Dr. Schwagmeyer, who had the immediate care of the case, reported to me: "Patient quite comfortable, very little fever, more appetite, tongue moist, complains of but little pain, has had several hours' quiet sleep." From this time on, the progress toward recovery was steady, and in a little less than two months the wound was entirely closed.

It is, perhaps, a significant fact, with reference to the cause of the tarsal disease, that just before this complete cicatrization occurred, the man had quite a severe acute periostitis of the *left* tibia in its upper third, which soon disappeared under the administration of full doses of the iodide of potassium.

This case, so far as I have been able to ascertain, was the first in which the entire tarsus was taken away.

In Jaesche's case, which was reported nine years previously, there was left of the os calcis "the posterior part of the crust in form of half an egg-shell," to which the tendo Achillis was attached.

At the present time the man (not using even a cane) walks firmly and rapidly without limp, and can easily and quickly go up and down stairs, or climb up on to his wagon-seat. There is a half-inch shortening of the leg; the lateral movements of the foot are slightly limited; and the range of flexion and extension of foot on leg is lessened about one-fourth. The sole is flattened, but there is no lateral deviation of the foot or elevation of border. No re-formation of bone seems to have taken place. The foot is three inches shorter than its fellow (7 in.—10 in.); the circumference at instep level is one-fourth inch greater (9 in.—$8\frac{3}{4}$ in.); measured over the heel and just below the malleoli the circumference is one-fourth inch less ($11\frac{3}{4}$ in.—12 in.).

Case II.—E. S., æt. 20, German. First seen in Cincinnati Hospital, April 1, 1876. Had had for over two years disease of the right tarsus. Had already been twice operated upon by Dr. B. F. Miller, the "scaphoid and most of the astragalus" having been taken away in April, 1875, and three and a half months later "some small pieces of necrosed bone and a part of the os calcis." Examination of the foot showing that much, if not all, of what remained of the tarsus was carious, on the 8th of April I removed the whole of it through an external lateral incision.

The after-treatment was similar to that of Case I. As in that case there was an early rise of temperature (103° F.) at the end of the first twenty-four hours, with quick decline. Healing went on steadily, but slowly, and it was nearly six months before the man was able to readily move about without crutches.

Seven and a half months after the operation he could walk very well, and was doing duty as a nurse in the hospital ward. He now walks with ease and quite rapidly; never uses a cane; can run up and down stairs; and says that, so far as he can tell, the ankle movements are as good as ever. Their range does not seem to be any less than that of the sound ankle. Lateral movement is a little restricted. Owing, undoubtedly, to too great use of the foot soon after passing from under my observation, the leg bones have been crowded downwards and inwards; but no elevation of the outer border of the foot has taken place. The leg is shortened three-fourths of an inch, and the foot two and a half inches (7 in.—9½ in.). The instep circumference is one and a quarter inch less (7¼ in.—8½ in.); that over heel and below the malleoli two inches less (9½ in.—11½ in.); that at level of base of toes one inch less (7¼ in.—8¼ in.) than the like measurements of the left foot.

Satisfactory as has been the end-result in each of these two excisions, has it been merely surgical good fortune, or have we a reasonable prospect of securing the like in similar conditions of the foot? Though the number of operations tabulated is limited, it is yet, I believe, sufficiently large to warrant the deducing therefrom of certain general conclusions; especially since they have been made by many operators in various places and under diverse conditions, as respects time, age, and personal character.

Excision of entire Tarsus.

No.	Operator.	Authority.	Sex	Age.	Date.	Cause.	Bones removed.	Result as to Life.	Result as to Usefulness of foot.	Cause of death.	Remarks.
1	Conner	.....	M.	39	1875	Caries	Entire tarsus.	R.	Very good	.....	½ in. shortening of leg, 3 in. shortening of foot.
2	Conner	.....	M.	20	1876	Caries	Entire tarsus.	R.	Very good	.....	¾ in. shortening of leg, 2¾ in. shortening of foot.
3	Jones, H. M.	London Lancet, July 2, 1881.	F.	Youthful.	1878	Caries	Entire tarsus.	R.	Very good	.....	Malleoli removed. "Appears to be a new osseous growth."

Total number of cases, 3. Recoveries: very good, 3.

Excision of nearly the entire Tarsus.

No.	Operator.	Authority.	Sex	Age.	Date.	Cause.	Bones removed.	Result as to Life.	Result as to Usefulness of foot.	Cause of death.	Remarks.
4	Jaesche	Langenbeck's Archiv f. Klin. Chirg. B. 8, 162. 1866.	M.	22	186–	Caries	Entire tarsus with the exception of posterior-inferior part of os calcis—base of 2d metatarsal.	D.	....	Pyæmia on 18th day.	
5	Cheever	Boston City Hosp. Reports, 2d series, p. 285.	F.	5	1875	Caries	Entire tarsus with the exception of astragalus.	R.	Very good	.....	"Left the hospital in about 4 months, walking well."
6	Kappeler	Deutsche Zeitschr. f. Chirurg. 188–, p. 432 *et seq.*	F.	19	1875	Caries	Entire tarsus with the exception of the posterior part of os calcis.	R.	Very good		
7	Kappeler	Op. cit.	F.	26	1877	Caries	Entire tarsus with the exception of one-half of the os calcis. Articular end of tibia and of fibula.	R.	Fair	.....	2½ years later had 20° active and 30° passive movement of ankle. Toes freely movable, and properly placed. Still used crutch in walking.

Total number of cases, 4. Recoveries: very good, 2; fair, 1 = 3. Deaths, 1.

Excision of nearly the entire Tarsus with Metatarsus.

No.	Operator.	Authority.	Sex	Age.	Date.	Cause.	Bones removed.	Result as to Life.	Result as to Usefulness of foot.	Cause of death.	Remarks.
8	Conner	Amer. Journal Medical Sciences, July, 1875.	M.	33	1874	Caries	Entire tarsus with the exception of part of os calcis and part of astragalus. Entire metatarsus.	R.	Very good	.....	Died eight years later of typhoid fever.

Total number of cases, 1. Recoveries: very good, 1.

Excision of nearly the entire Tarsus with part of Metatarsus.

No.	Operator.	Authority.	Sex	Age.	Date.	Cause.	Bones removed.	Result as to Life.	Result as to Usefulness of foot.	Cause of death.	Remarks.
9	Kappeler	Op. cit.	F.	46	1877	Caries	Entire tarsus with the exception of part of os calcis. Fifth metatarsal bone.	R.	Fair	.....	"With the aid of a cane walks very well." Osteitis in other foot.

Total number of cases, 1. Recoveries: fair, 1.

Excision of Posterior Tarsus.

10	Wakley	London Lancet, July 1, 1848.	M.	23	1847	Caries	Posterior tarsus with the malleoli.	R.	Very good	.....	Healed in four weeks.
11	Ring	Hancock, Anatomy and Surgery of Human Foot, p. 431.	F.	13	1855	Caries	Posterior tarsus.	R.	Good		
12	Heyfelder, J. F.	Heyfelder's Table.	M.	17	1858	Caries	Posterior tarsus.	R.	Failure	.....	Amputation on 24th day.
13	Cabot	Records Med. Imp. Soc. (Boston), v. 4, p. 65.	M.	Adult	1859	Necrosis	Posterior tarsus.	..	Uncertain	.....	At time of report "patient was doing well."
14	Whitehill, J. C.	Med. and Surg. Hist. War of Rebel'n. Surg. Vol. part 3, p. 592.	M.	26	1863	Gunshot wound	Posterior tarsus.	R.	Good	.....	Eighteen years later the limb was shortened 2½ inches.
15	Morton, Thos. G.	Amer. Journ. Med. Sci., April, 1871, p. 424.	M.	13	1869	Caries	Posterior tarsus.	R.	Very good	.....	"Very perfect recovery" as respects motion and usefulness. About 1 inch shortening.
16	Humphry, F. A.	St. Barth. Hosp. Rep., vol. x. p. 377.	M.	9	1874	Caries	Posterior tarsus.	R.	Very good	.....	"With a high-heeled boot he can, with a slight limp, walk well, and without a stick." 1½ inch shortening of leg.
17	Tiling	Med. & Surg. Hist. War Rebel'n. Surg. Vol. part 3, p. 611.	M.	28	1876	Gunshot wound	Posterior tarsus.	D.	....	Gangrene—on 10th day.	Amputation on 5th day.

Total number of cases, 8. Recoveries: very good, 3; good, 2; uncertain, 1: failure, 1 = 7. Deaths, 1.

Excision of part of Posterior Tarsus with Ankle-Joint.

18	Schüller	Deutsche Zeitschrift f. Chirurg. 1878, p. 303, *et seq.*	M.	10	1876	Caries	Ankle-joint with greater part of astragalus and of os calcis.	R.	Very good	.....	Same patient: operation on one side in September, on the other in November. Fifteen months later patient walked about 7½ miles without difficulty.
19	Hueter	Deutsche Zeitschrift f. Chirurg. 1878, p. 393 *et seq*	M.	10	1876	Caries	Ankle-joint with greater part of astragalus and of os calcis.	R.	Very good	.....	

Total number of cases, 2. Recoveries: very good, 2.

Excision of Posterior Tarsus with part of Anterior Tarsus.

No.	Operator.	Authority.	Sex	Age.	Date.	Cause.	Bones removed.	Result as to			Remarks.
								Life.	Usefulness of foot.	Cause of death.	
20	Teale	Med. Times and Gaz., May 27, 1854.	M.	24	1853	Caries	Posterior tarsus and cuboid.	R.	Fair	.....	"Walked about with the assistance of a stick." Ankle motions good at expiration of ten months.
21	Teale	Med. Times and Gaz., Oct. 13, 1855, and Hancock, op. cit. p. 433.	F.	24	1855	Caries	Posterior tarsus, cuboid, and scaphoid.	R.	Very good	.....	Could do servant's work after six months. Could walk without stick.
22	Swain	British Med. Journ., Jan. 7, 1871.	M.	11	1864	Caries	Ankle-joint, posterior tarsus, and part of scaphoid and of cuneiforms.	R.	Very good	.....	Foot "well nourished and quite painless." 1¼ inch shortening. "Can walk any reasonable distance without distress."
23	Lehman	Deutsche Klinik, 1870, p. 10.	M.	40	1867	Caries	Ankle joint, posterior tarsus, and scaphoid.	R.	Very good	.....	After ten months patient was completely satisfied with the condition of his foot.
24	Fayrer	Med. Times and Gaz., July 31, 1869.	M	9	1869	Caries	Posterior tarsus and scaphoid.	R.	Uncertain	.....	At time of report could put foot on the floor, and bear considerable weight upon it. "In fair way to make excellent recovery."
25	Goltdammer	Berl. Klin. Wochensch. 1871. 8, p. 151.	M.	Adult	1870	Gunshot wound	Posterior tarsus and scaphoid.	R.	Good		
*26	Krohn	Kappeler's Table.	M.	10	1871	.....	Posterior tarsus and internal malleolus.	R.	Good	.....	Subperiosteal resection. Extensive reformation of bone. Some limp in walking. No pain.
27	Kappeler	Op. cit.	M.	8	1879	Caries	Posterior tarsus, scaphoid, and part of cuboid.	D.	....	Amyloid disease of liver and spleen.	Lived three months.
28	Mikulicz	Langenbeck's Archiv f. Klin Chir., B. 20, 1881, S. 494 *et seq.*	M.	23	1880	Syphilitic ulcerat'n of soft parts of heel.	Ankle-joint, posterior tarsus, and proximal half of scaphoid and cuboid.	R.	Very good	.....	Osteoplastic resection. After fourth month could walk without shoe, and bear the entire weight of body on foot.
29	Conner	.....	M.	52	1882	Necrosis	Posterior tarsus and part of cuboid.	D.	....	Amyloid disease of intestines	Lived three months.

Total number of cases, 10. Recoveries: very good, 4; good, 2; fair, 1; uncertain, 1 = 8. Deaths, 2.

* This case should have appeared in table of "Excisions of Posterior Tarsus," following No. 15.

Excision of part of Posterior Tarsus with part of Anterior Tarsus.

No.	Operator	Authority.	Sex	Age	Date.	Cause.	Bones removed.	Result as to Life.	Result as to Usefulness of foot.	Cause of death.	Remarks.
30	Durand	Heyfelder's Table.	M.	14	1745	Caries	Astragalus, part of os calcis, cuboid.	R.	Very good	.....	Afterwards served as a soldier for thirteen years.
31	Liston	Edinb Med. Journ., Jan. 1821.	F.	12	1818	.....	End of tibia, astragalus, scaphoid, and two cuneiforms.	R.	Good	.....	"Complete and lasting cure without much deformity or lameness."
32	Liston	Blackmann's Mott's Velpeau, v. 2, p. 483.	..	....	1832	.....	A part of the astragalus, scaphoid, and two cuneiforms.	R.	Failure	.....	"Did not succeed."
33	Jaeger	Heyfelder's Table.	..	....	1832	Caries	Astragalus and scaphoid.	R.	Uncertain		
34	Robert	Vierteljahrschrift f d. Praktische Heilkunde, 1855, B. 2, p. 1 *et seq.*	F.	34	1851	Caries	Os calcis and cuboid.	R.	Very good	.....	Relapse 1½ years later. Death from phthisis.
35	Textor	Heyfelder's Table.	M.	23	1852	Caries	Ankle-joint, astragalus, part of os calcis, scaphoid.	R.	Very good		
36	Robert	Op. cit.	M.	58	1852	Caries	Anterior half of os calcis, cuboid, and scaphoid.	R.	Good		
37	Robert	Op. cit.	F.	35	1853	Caries	Anterior half of os calcis, greater part of cuboid.	R.	Good	.....	"Useful foot."
38	Cotton	Med Times and Gaz., June 16, 1855.	M.	30	1855	Caries	Ankle-joint, astragalus, part of os calcis, of scaphoid, and of the three cuneiforms.	R.	Fair	.....	Recovery in ten weeks.
39	Cock	Med. Times and Gaz., Feb. 21, 1857.	F.	21	1856	Caries	Part of astragalus and of os calcis with scaphoid, cuboid, and one cuneiform.	R.	Failure	.....	At time of report, some dead bone remaining, and foot much misshapen.
40	Bradford, J. T.	Gross' "Surgery," vol. ii. p. 1087.	M.	15	1857	Caries	Os calcis, small part of astragalus and cuboid.	R.	Very good	.....	"Excellent recovery."
41	Heusser	Deutsche Klinik, 1860.	..	....	1860	Caries	Os calcis, scaphoid, and first cuneiform.	R.	Failure	.....	Subsequent amputation.
42	Humphry, G. M.	Med. Times and Gaz., April 27, 1861.	M.	12	1860	Caries	Ankle joint, astragalus, and scaphoid.	R.	Failure	.....	Subsequent amputation.
43	Langenbeck	Vincent, Thèse de Paris, 1870, p. 73.	F.	11	1861	Caries	Os calcis, cuboid, part of astragalus.	R.	Very good	.....	Complete regeneration of bone.
44	Billings	Med. & Surg. Hist. War Reb. Surg. Vol. part 3, p. 688.	M.	52	1862	Incised wound	External malleolus, astragalus, and part of scaphoid.	R.	Fair	.....	"Could walk with aid of cane."
45	Gant	London Lancet, July 23, 1864.	M.	60	1862	Caries	Os calcis, cuboid, and part of third cuneiform.	R.	Very good	.....	"Ultimately walked without crutch or cane, and had a sound and useful foot."
46	Holmes, T.	London Lancet, May 27, 1865, p. 564.	F.	10	1863	Caries	Astragalus and scaphoid.	R.	Very good	.....	"Can walk perfectly, limp hardly perceptible."

Excision of part of Posterior Tarsus with part of Anterior Tarsus.—*Concluded.*

No.	Operator.	Authority.	Sex	Age.	Date.	Cause.	Bones removed.	Result as to Life.	Result as to Usefulness of foot.	Cause of death.	Remarks.
47	Humphry, G. M.	London Lancet, Aug. 6, 1864	M.	Youthful	1864	Caries	Part of astragalus, part of os calcis, part of cuboid, part of first cuneiform, with whole of scaphoid	R.	Very good	.....	Four years later could run about as though nothing had happened.
48	Humphry, G. M.	London Lancet, Aug. 6, 1864.	..	....	....	.....	Ankle-joint, astragalus, and scaphoid.	R.	Failure	.....	Subsequent amputation.
49	Simon	Kappeler's Table.	..	....	1866	.....	Ankle-joint, astragalus, anterior part of os calcis, scaphoid, and cuboid.	R.	Good	.....	"Happy result."
50	Mulvany	London Lancet, Nov. 6, 1869.	M.	29	1866	Caries	Ankle-joint, astragalus, ½ of os calcis, ¾ of scaphoid.	R.	Very good		
51	Sayre	Bellevue Hosp. Reports, 1870, p. 116.	M.	27	1868	Caries	Part of astragalus, great part of os calcis, scaphoid and other bones.	R.	Very good	.....	"Walks without limping."
52	Jayaker	Med. Times and Gaz., Oct. 30, 1869.	M.	30	1869	Caries	Part of astragalus, scaphoid, and two cuneiforms.	R.	Good	.....	Sound and useful limb.
53	Langenbeck	Langenbeck's Archiv f. Klin. Chir., B. 16, 2, p. 246.	M.	Adult	1870	Gunshot wound	Ankle-joint, astragalus, part of os calcis, cuboid.	R.	Very good	.....	"Walks for hours without limping." Anchylosis.
54	Walter, A. G.	Culbertson's Table.	F.	10	1870	Caries	Ankle-joint, astragalus, almost all of the os calcis, scaphoid, part of cuboid	R.	Very good	.....	Foot "useful to the fullest extent." Less than one inch shortening.
55	Walter, A. G.	Culbertson's Table.	F.	43	1872	Caries	Greater part of astragalus and of os calcis, part of cuboid and of scaphoid.	R.	Very good	.....	"Perfect usefulness of the joint and foot."
56	Levis	Phil. Med. Times, vol. iv. p. 310.	M.	17	1873	Caries	Ankle-joint, astragalus, part of scaphoid and of cuboid.	R.	Uncertain	.....	Seven months after the operation patient was "able to bear some weight on the foot;" was using crutches.
57	Nicholls	Holmes's "System of Surg." Am. ed., vol. iii. p. 236.	..	....	....	.....	Almost the entire astragalus, os calcis, part of scaphoid.	R.	Very good	.....	Two operations. "Patient can walk quite well, and with a hardly perceptible limp."
58	Homans, C. D.	Med. News (Phila.), Feb. 24, 1883.	F.	20	1882	Caries	Astragalus, nearly the whole of os calcis, nearly all the tarsal bones.	R.	Failure	.....	Amputation two months later, though healing was progressing favorably.

Total number of cases, 29. Recoveries: very good, 14; good, 5; fair, 2; failures, 6; uncertain, 2 = 29.

Excision of part of Posterior Tarsus with Anterior Tarsus and Part of Metatarsus.

No.	Operator.	Authority.	Sex	Age.	Date.	Cause.	Bones removed.	Result as to Life.	Result as to Usefulness of foot.	Cause of death.	Remarks.
59	Dunn	Med. Chir. Trans., vol. xi. p. 357.	M.	14	1816	Caries	Very small part of astragalus, anterior tarsus, bases of 2d and 3d metatarsals.	R.	Very good	.....	Bones removed at two operations. 4 years later 1½ inch shortening. In less than a year after the operation the boy walked 9 miles in one day.
60	Lauenstein	Proceedings of 12th Congress of German Surgeons.	F.	54	1881	Caries	Part of os calcis, anterior tarsus, bases of 4th and 5th metatarsals.	R.	Very good	.....	Almost imperceptible limp; ½ inch shortening of foot. Some reproduction of bone.

Total number of cases, 2. Recoveries: very good, 2.

Excision of part of Posterior Tarsus, part of Anterior Tarsus, and part of Metatarsus.

No.	Operator.	Authority.	Sex	Age.	Date.	Cause.	Bones removed.	Result as to Life.	Result as to Usefulness of foot.	Cause of death.	Remarks.
61	Moreau, Sr.	Moreau, Resection des Os, p. 110.	M.	40	1788	Caries	Part of os calcis, cuboid, 3d cuneiform, and part of 4th and 5th metatarsals.	R.	Very good	.....	"Perfect cure."
62	Beck	Langenbeck's Archiv f. Klin. Chir., B. 5, p. 238.	M.	17	1864	Caries	Os calcis, cuboid, and two metatarsals.	R.	Very good	.....	Could do mountain climbing "without inconvenience."
63	Czerny	Volkmann's Klin. Vortr. No. 76.	M.	17	1874	Caries	Os calcis, cuboid, bases of 4th and 5th metatarsals.	R.	Uncertain	.....	Under treatment at time of report.

Total number of cases, 3. Recoveries: very good, 2; uncertain, 1 = 3.

Excision of Anterior Tarsus.

No.	Operator.	Authority.	Sex	Age.	Date.	Cause.	Bones removed.	Result as to Life.	Result as to Usefulness of foot.	Cause of death.	Remarks.
64	de Housse	Thomas. Thèse de Paris, 1853, No. 253.	M.	36	1775	Caries	Anterior tarsus.	R.	Very good	.....	Healed in two months; regeneration of bone. Walked "six leagues a day without aid and without fatigue." Two operations.
65	Jaesche	Langenbeck's Arch. f. Klin. Chir., 1866, B. 8, p. 162.	M.	51	1863	Caries	Anterior tarsus.	D.	....	Cirrhosis of liver and pericarditis	Two operations. Lived 14 months.
66	Bryant	"Surgery," p. 882. 3d Amer. ed.	M.	8	....	.....	Anterior tarsus.	R.	Very good	.....	"Excellent foot."
67	Socin	Deutsche Zeitschr. f. Chir., 1879, p. 350 *et seq.*	M	15	1879	Caries	Anterior tarsus.	R.	Good	.....	Healed after ten months.

Total number of cases, 4. Recoveries: very good, 2; good, 1; death, 1 = 4.

Excision of Anterior Tarsus with Metatarsus.

No.	Operator.	Authority.	Sex	Age.	Date.	Cause.	Bones removed.	Result as to Life	Result as to Usefulness of foot.	Cause of death.	Remarks.
68	Conner	Amer. Journ. Med. Sci., July, 1875.	M.	31	1874	Caries	Anterior tarsus, entire metatarsus, except small part of 1st metatarsal.	D.	....	Pyæmia	Two operations sixteen days apart. Lived eleven days after second operation.

Total number of cases, 1. Deaths, 1.

Excision of part of Anterior Tarsus with Metatarsus.

69	Kappeler	Op. cit.	F.	26	1868	Caries	Cuboid, 3 cuneiforms, and all the metatarsals.	R.	Very good		

Total number of cases, 1. Recoveries: very good, 1.

Excision of Anterior Tarsus with part of Metatarsus.

70	Moreau, Jr.	Blackman's Mott's Velpeau, 2, 483.	..	....	....	.....	Anterior tarsus, bases of 3d, 4th, and 5th metatarsals.	R.	Failure	.....	Caries reappeared in other bones before cicatrization took place.
71	Champion	Moussu, Thèse de Strasbourg, 1863, 2d ser., No. 748.	..	....	1839	Caries	Anterior tarsus, with bases of 3d, 4th, and 5th metatarsals.	R.	Failure	.....	Caries reappeared "in other bones of the foot before cicatrization took place."
72	Laugier	Thomas, Thèse de Paris, 1853, p. 253.	F.	47	1853	Caries	Anterior tarsus, bases of 2d, 3d, and 4th metatarsals.	D.	....	Phthisis 1 month later	"Examination of the uncicatrized foot showed that the ultimate result would doubtless have been satisfactory."
73	Bigelow, H. J.	Records Med. Imp. Soc. (Boston), v. ii. p. 342.	M.	44	1855	.....	Anterior tarsus, with bases 2d and 3d metatarsals.	D.	....	Exhaustion and tuberculosis	Lived 4½ months. "Health improved for 3 months, when his mind suddenly gave way; tendency to diarrhœa supervened, and he died of tuberculous disease."
74	Watson, P. H.	Edinb Med. Journ., May, 1874.	M.	19	1870	Caries	Anterior tarsus, with bases of metatarsals.	R.	Very good	.....	"Walked almost without defect."
75	Watson, P. H.	Op. cit.	..	....	....	.....	Anterior tarsus, with bases of metatarsals.	R.	Good		
76	Watson, P. H.	Op. cit.	..	....	....	.....	Anterior tarsus, with bases of metatarsals.	R.	Good		
77	Watson, P. H.	Op. cit.	..	....	....	.....	Anterior tarsus, with bases of metatarsals.	R.	Good		
78	Watson, P. H.	Op. cit.	..	....	....	.....	Anterior tarsus, with bases of metatarsals.	R.	Good		

EXCISION OF ANTERIOR TARSUS WITH PART OF METATARSUS.—*Concluded.*

No.	Operator.	Authority.	Sex	Age.	Date.	Cause.	Bones removed.	Result as to Life.	Result as to Usefulness of foot.	Cause of death.	Remarks.
79	Watson, P. H.	Edinb. Med. Journ., May, 1874.	..	....	....	.....	Anterior tarsus, with bases of metatarsals.	R.	Failure	.....	Subsequent amputation.
80	Bruns	Langenbeck's Archiv f. Klin. Chir., B. 19. s. 058.	F.	36	1874	Caries	Anterior tarsus, with bases of metatarsals.	R.	Uncertain	.....	Discharged in 7 weeks. "Steps on foot without difficulty."
81	Dawson	Cincinnati Clinic, Jan. 1, 1870.	F.	6	1874	Caries	Anterior tarsus, with bases of metatarsals.	R.	Very good		
82	Watson, P. H.	Edinb. Med. Journal, April, 1875.	..	....	....	.....	Anterior tarsus, with bases of metatarsals.	..	....	.....	Result not stated. Subperiosteal operation.
83	Socin	Loc. cit.	F.	9	1879	Caries	Anterior tarsus, with bases of metatarsals.	R.	Uncertain	.....	Wound firmly healed. Died of typhoid fever 6 months after the operation.

Total number of cases, 14. Recoveries: very good, 2; good, 4; failures, 3; uncertain, 2 — 11. Deaths, 2; result unknown, 1.

EXCISION OF PART OF ANTERIOR TARSUS.

84	Meyer	Heyfelder's Table.	F.	37	1845	Caries	Scaphoid, 1st and 2d cuneiforms.	R.	Uncertain	.....	"Partial success."
85	Rayner	Hancock, Anat. and Surg. Human Foot, p. 439.	M.	16	1845	Caries	3 cuneiforms.	R.	Uncertain	.....	"Partially successful, but the patient died some months afterwards of disease of his lungs."
86	Skey	London Lancet, May 28, 1859.	M.	18	1858 } 1859 }	Necrosis	Greater part of the second row of tarsal bones.	R.	Uncertain	.....	Two operations.
87	Bryant	"Surgery," p. 982, 3d Am. ed.	..	....	....	.....	Cuboid, and three cuneiforms.	R.	Very good	.....	"Excellent foot."
88	Ownes	Culbertson's Table.	M.	56	1867	Necrosis	Cuboid, external and middle cuneiforms.	R.	Good	.....	"Good use of limb and foot."
89	Bruns	Langenbeck's Archiv f. Klin. Chir., B. 19, 058.	M.	17	1875	Caries	Cuboid and 3d cuneiform	D.	....	Amyloid disease of kidneys, spleen, and intestines	Died in ten days.
90	Socin	Loc. cit.	F.	27	1875	Caries	Scaphoid and 3 cuneiforms.	R.	Good	.....	After several years complete healing.
91	Hueter	Deutsche Zeitschr. f. Chir., 1878, p. 513 *et seq.*	M.	26	1876	Caries	Cuneiform bones and part of cuboid.	R.	Fair	.....	Discharged well in 2½ months. Walked with cane. Regeneration of bone.

Total number of cases, 8. Recoveries: very good, 1; good, 2; fair 1; uncertain, 3 — 7. Deaths, 1.

Excision of part of Anterior Tarsus with part of Metatarsus.

No.	Operator.	Authority.	Sex	Age.	Date.	Cause.	Bones removed.	Result as to Life.	Result as to Usefulness of foot.	Cause of death.	Remarks.
92	de la Motte	Moussu, Thèse de Strasbourg, 1863, 2d ser., No. 718.	..	....	....	Necrosis	2d and 3d cuneiforms and part of the 5th metatarsal.	R.	Good		
93	Bruns	Kappeler's Table.	..	....	1849	Caries	1st and 2d cuneiforms; 1st and 2d metatarsals.	R.	Uncertain	.....	Healed within three months.
94	Massarani	Kappeler's Table.	..	....	1852	Caries	1st and 2d cuneiforms; 1st metatarsal.	R.	Good	.....	Healed by first intention, and 1 month later patient walked without crutch.
95	Smith	Heyfelder's Table.	..	....	1856	Caries	2d and 3d cuneiforms; 4th and 5th metatarsals.	R.	Good		
96	Küchler	Deutsche Klinik, 1859, No. 41, p. 413.	M.	26	1857	Caries and necrosis	1st and 2d cuneiforms; 1st and 2d metatarsals.	D.	....	Dropsy (Amyloid disease?)	
97	Wright	Hancock, Anat. & Surg. Human Foot, p. 434.	..	....	....	.....	Scaphoid, 3 cuneiforms, 2 metatarsals.	R.	Uncertain		
98	Billroth	Kappeler's Table.	M.	26	1860	Caries	Part of cuboid, 3d cuneiform, 5th metatarsal, part of 4th metatarsal.	R.	Very good	.....	Partial regeneration of bone believed to have occurred.
99	Michel	Moussu, Thèse de Strasbourg, 1863, 2d ser., No. 718.	M.	32	1862	Caries	Anterior half of cuboid, 2d and 3d cuneiforms, bases of 2d, 3d, 4th, and 5th metatarsals.	R.	Very good	.....	After nine months able to do farm work. Could walk from 12 to 15 miles without fatigue.
100	Sauger, E. F.	Med. and Surg. Hist. War Reb. Surg. Vol. Part 3, p. 623.	M.	Adult	1863	Gunshot wound	2d cuneiform, parts of 1st and 3d cuneiforms, base of 2d metatarsal.	R.	Good		
101	Kade	St. Petersb. Med. Zeit., 1864, 7, 183.	M.	Adult	1864	Gunshot injury	Cuboid, 2d and 3d cuneiforms, great part of 4th and 5th metatarsals.	R.	Good	.....	Walked after three months.
102	Kade	St. Petersb. Med. Zeit., 1864, 7, 183.	M.	Adult	1864	Necrosis	Scaphoid, 1st and 2d cuneiforms, greater part of 1st metatarsal.	D.	....	Pyæmia	Lived six weeks.
103	Forster, C.	Guy's Hosp. Rep., ser. 3, vol. xvii. p. 387.	M.	4	1871	Caries	Scaphoid, internal cuneiform, part of 1st metatarsal.	R.	Good	.....	Left hospital after four months. "Gets about very well."
104	Kappeler	Op cit.	F.	27	1871	Caries	Scaphoid, 3 cuneiforms, part of cuboid, and posterior third of 2d and 3d metatarsals.	R.	Very good		
105	Holmes, T.	Trans. Clin. Soc. 5, 207.	M.	Middle age	1872	Caries	Entire tarso-metatarsal articulation and 2d metatarsal.	R.	Good	.....	Two operations.
106	Nesbitt	Brit. Med. Journ., Dec. 14, 1878.	F.	8	1878	Caries	Cuboid, 2d and 3d cuneiforms, 2d and 4th metatarsals, and later 3d metatarsal.	R.	Very good	.....	Healed in 4½ months.

EXCISION OF PART OF ANTERIOR TARSUS WITH PART OF METATARSUS.—*Concluded.*

No.	Operator.	Authority.	Sex	Age.	Date.	Cause.	Bones removed.	Result as to Life.	Result as to Usefulness of foot.	Cause of death.	Remarks.
107	Socin	Op. cit.	F.	14	1879	Caries	Scaphoid, 2d and 3d cuneiforms, base of 2d metatarsal.	R.	Very good	.....	After two years "perfect healing."
108	Schüssler	Kappeler's Table.	M.	10	1880	Caries	Cuboid, 2d and 3d cuneiforms, 5th metatarsal, and large pieces of 2d, 3d, and 4th metatarsals.	R.	Very good	.....	Final cicatrization six months after the operation. Re-formation of bone. "Function of foot normal."

Total number of cases, 17. Recoveries: very good, 6; good, 7; uncertain, 2 = 15. Deaths, 2.

RECAPITULATION.

Total number of cases			108
Recoveries:	very good	45	
	good	23	
	fair	6	96 (88.89 per ct.)
	failures	10	
	uncertain	12	
Result unknown		1	(0.93 per ct.)
Deaths		11	(10.18 per ct.)
		108	108

Total number of operations performed	prior to 1860		31
" " " "	subsequent to 1860		64
" " " "	date uncertain		13
			108

The three questions to be considered are:—

1. Is excision a safe operation, or at least attended with no greater mortality than the alternative—amputation?

2. Is it likely to put an end to the disease, or is recurrence of the morbid process in the unremoved bones of the foot to be expected?

3. Will the patient, after recovery from the operation, be left with a serviceable limb?

Of the 108 cases collected, 11 died (10.18 per cent.), but 1 of them died of amyloid disease in ten days, and 1 of phthisis in a month, and, if we deduct these cases, in neither of which was the fatal result due to the operation, the mortality is reduced to 8.33 per cent.; and if Tiling's case is thrown out, in which, after gunshot injury, intermediary excision was made, followed by amputation five days later, and by death from gangrene in another five days, the percentage is reduced to 7.4.

Of the 9 fatal cases (including Tiling's) in 4 at the lowest a Syme amputation and in 5 a Chopart would have had to be performed; or, to put it in other words, out of 63 cases, in which the alternative was an ankle-joint amputation, 4 died (6.35 per cent.); and out of 45, in which a middle tarsal removal might have been made (though without doubt in some of them a Syme or Pirogoff amputation would have been performed), 5 died (11.11 per cent.).

The Syme operation mortality for caries is about 6 to 8 per cent. (at least 10 per cent. according to Delorme), the Pirogoff probably about the same, and the Chopart perhaps 4 per cent., though Schede has placed it as high as 10 per cent.

Examination of the table further shows, that while it is true that excision is attended with very much less mortality in young subjects, the opinion expressed by certain writers that it should be confined to them, is not warranted by the facts.

Of 87 cases, the ages of which are given, 28 were not over 15 years of age, of whom but 1 died (3.57 per cent.), 18 were between 15 and 25 years old, of whom 2 died (11.11 per cent.), and 41 were older, of whom 8 died (19.51 per cent.).

Of those in whom either the entire tarsus or at least the whole

of one of its great divisions was taken away, the death-rate was, under 15 years of age, 6.67 per cent., over 15 and under 25, 10 per cent., and over 25, 27.27 per cent.

Of the 6 operations for gunshot injury the subjects of which were all adults, 1 (Tiling's) resulted in death (16.67 per cent.). In 4 of these 6 cases (in which is included the fatal one), an ankle-joint amputation would otherwise, without doubt, have been performed; an operation that for gunshot wound in our late war had a mortality rate of 25.1 per cent.

As respects the preservation of life then, excision of the whole tarsus, or of one of its great divisions, is not much, if any, more dangerous than an ankle-joint amputation, and not very much more so than a middle tarsal operation.

It has, moreover, the advantage of permitting, if necessary, of the subsequent removal of the foot, which, in the cases tabulated, was performed seven times with but a single resulting death. That the disease is very unlikely to reappear is shown by the fact, that in only 3 cases (Champion's, Robert's, and Moreau's) did such recurrence take place. This fact, however, is not so strange as it may at first seem, as it is in direct accordance with the recognized law that the more thorough the removal of diseased bone the less will be the likelihood of a relighting up of the bone inflammation.

By far the most important question of the three proposed is that which has reference to the functional value of the saved foot. Is its usefulness likely to be sufficiently great to warrant the taking of a somewhat increased risk of life, and in subjecting the patient to the necessarily much greater duration of the period of healing—a period that, though it may be as short as four weeks (Wakley's), may occupy more than twice as many months (Socin's), or even half as many years?

In 13 of the 97 cases that did not die, the reports, either because imperfect or made soon after the performance of the operation, do not clearly indicate the end-result. In 10 the operations were failures, subsequent amputation being required in 7 of them; in 1 of these 7, however (Homan's), the healing is stated to have been progressing favorably, the foot having been

removed at the patient's request; and in another (Watson's), made out of regard to "the patient's sensitiveness and weakness," the operator was "chagrined to find that there was no condition which should not have admitted of sound cicatrization."

In 6 cases I have considered the result as *fair*, five of the individuals being able to walk about, but requiring the aid of a cane, and in the sixth there was considerable motion at the ankle-joint, and the toes were freely movable, though the woman, two years and a half after the operation, continued to use a crutch in walking.

Forty-five times the ultimate functional value of the foot was *very good*, and twenty-three times more, *good;* and no small part of the twenty-three *good* results might very properly, I think, have been included with the *very good.*

Put in figures, we may say that, of the 108 operations, 10.18 per cent. resulted fatally (more justly, as has already been stated, 8.33 per cent.). Of the 95 cases, the end-results of which are known, 10.53 per cent. were failures; 6.32 per cent. left the subjects of them able to walk with a cane or crutch; in 24.21 per cent. there was, after complete consolidation had taken place, no pain nor tenderness, little or no limp, and the individuals were not prevented by the condition of the foot from earning a livelihood; and in 47.37 per cent. the result was so good that the gait was not a bad one, the support of the body was firm, and locomotion was so easy and perfect that the individuals could, without special fatigue, walk long distances; even six leagues a day, if we may believe the report of de Housse's case, certainly twelve to fifteen miles a day, as Michel's patient ultimately did.

As respects shape, though it must be changed, ordinarily materially so, the foot being shortened, broadened, and flattened, still no form of talipes need be developed if due care be taken to maintain a proper position during the period of healing, especially the later part of it.

Surely there was not in any of these sixty-eight cases that "weak, flabby, and deformed condition" of the foot that Roser would have us believe is to be expected, even when rapid heal-

ing occurs; and there was not one of the individuals that did not have a "a firm weight-supporting foot, the serviceableness of which would compare with that of an artificial foot after a leg amputation." One of Humphry's cases ran about as though nothing had happened. One of Kappeler's patients, from whom he had three years previously removed the entire tarsus with the exception of the posterior part of the os calcis, was able to do full work in a stocking factory, walk over four miles, in a common shoe, without being tired, and as rapidly as others, and even dance, though not waltz.

Durand's patient later served thirteen years as a soldier. Beck's was able to do mountain climbing, and Langenbeck's walked for hours without limping. Each of my own patients whose cases I have reported in this paper earns his daily bread by his daily work, the one as a wagon driver, the other as a baker.

In the case which I reported eight years ago in *The American Journal of the Medical Sciences*, the man walked about the city without any difficulty; and if he did not perform regular work, it was because of his natural disinclination to do any more than he could help, and not because of the condition of his foot.

The ankle motions, even in certain of the complete or almost complete excisions, were to some extent preserved. In Walter's case there was said to be "perfect usefulness of the joint;" and one of my own patients declares that he does not perceive any difference in the movements of the two ankles. Kappeler reports that in two of his cases the arch of the foot was preserved, though in a modified form.

In none of the cases was the shortening of the limb so great that it could not be corrected by a thickened sole; being in two of the eight cases in which the amount has been noted one-half inch, in one three-quarters of an inch, in one one inch, in three one-and-a-half inch, and in one two-and-a-half inches.

Osseous regeneration to greater or less extent was believed to have taken place in eight cases (3, 26, 43, 60, 64, 91, 98, 108).

Examination of the appended table confirms me in the opinion I had already formed from the cases that had fallen under my

own observation, that the more extensive the removal of bone, the better the ultimate result. Münch has expressed himself to the same effect; and Kappeler has very properly declared that the larger the excision the more it resembles amputation, as respects the healing.

The very favorable result following many of the most extensive operations, makes me ready to believe the correctness of the report of Bilguer's case (which I have not included in the table) of removal, after gunshot injury, of "nearly all the bones of the foot," in which ultimately the "officer was enabled to walk and to resume his duties by means of a heel of double the usual thickness;" the truth of which Velpeau was evidently somewhat inclined to doubt.

As respects the method of operating in these tarsal excisions, no definite rules can be laid down. Ordinarily, a lateral incision or one on each side, while rendering the removal of the bones more difficult, will give ultimately the best result, a dorsal cut necessarily dividing the extensor tendons.

It is, however, very probable, that if the ends of such tendons be at once united by sutures, most, if not all, of the disadvantages of such section will be prevented; and even if left to make their attachments as best they may to the soft parts, a very excellent end-result may follow, as in my own case of removal of the tarsus and metatarsus.

Theoretically, the excision when for caries or necrosis should be made sub-periosteally, but, practically, it is very doubtful if the preservation of the periosteum will, in the majority of cases, be of any advantage to the patient; as much, if not all, of such retained membrane will usually be destroyed in the after long-continued suppuration of the wound. Very great advantage attends the use of the Esmarch bandage; and quite firm fixation of the foot and leg must be made.

Immobilization by the application of a plaster-of-Paris dressing, in the single case in which I made it, did not seem to answer as good a purpose as the use of the fracture box, or the tin or wire splint.

In determining in any given case whether or not excision

should be performed, the chief, if not the only thing to be taken into consideration, is the probable ability of the patient to endure the protracted period of healing.

Very early or quite advanced age is not a positive contra-indication; Cooper Forster's patient was but four years old; Gant's was sixty; both recovered with useful feet.

In very unhealthy and much debilitated subjects, amputation will generally be preferable to excision; as also in cases of extensive crushing of the foot.

Under other circumstances, as respects the cause of the existing disability and the general condition of the patient, I cannot but feel that enough has already been done by English, Continental, and American surgeons to show that the generally received opinion that extensive tarsal disease necessitates amputation is an incorrect one, or at least one that requires and should have reconsideration.

THE PRESIDENT. It seems to me that there can hardly be any discussion on this paper of Dr. Conner. It is so complete and so accurate that there hardly remains anything to be said.

TREPHINING OF THE STERNUM FOR REMOVAL OF A FOREIGN BODY FROM THE ANTERIOR MEDIASTINUM.

With the Report of the Case.

By S. MARKS, M.D.,
of Milwaukee, Wisconsin.

[Read June 1, 1883.]

I desire to call the attention of the Fellows of the Association to the following report of a case in which a foreign body was removed from the anterior mediastinum by trephining the sternum.

October 11, 1870, Captain B., a strong, robust man, aged thirty-four years, formerly a member of the 4th Ohio Vol. Inf., consulted me for what was supposed to be simple caries of the sternum, and gave the following imperfect history of his case:—

At one of the battles of the Wilderness, May 10, 1864, while in the act of rising from a recumbent posture, he was struck at a point a little above the centre of the breast bone by what was thought to be a Minié-ball. The force of the blow knocked him down, but he soon recovered from the shock sufficiently to walk to the rear, where he was examined by the field surgeons and told that he had been struck by a spent ball, which had not penetrated the bone. He suffered intense pain, the action of the heart was considerably disturbed, and the difficulty of breathing such as to compel him to constantly maintain the erect position.

May 11th he was removed from the field to one of the hospitals in Fredericksburg, Virginia, where he was examined by the surgeons in charge and the previous diagnosis confirmed. At this time the tissues over the sternum were very much swollen and there was great soreness through the chest, accompanied with more or less cough and expectoration mixed with blood.

Little or no change occurred in his symptoms until the fourth day after the injury, when a discharge of pus from the wound gave him great relief.

May 15th he was transferred to one of the hospitals in Washington, D. C., which one he does not remember, where he was again examined, not only by the surgeons in charge but by many others, all agreeing that he was suffering from the effects of a blow upon the outer surface of the sternum.

His condition slowly improved, and after a few days he was able to take some out-door exercise, but being thoroughly impressed with the idea that remaining in the hospital would cause death, he left it, without permission from the surgeons, some eight or ten days after admission, and went to his friends in Canton, Ohio, where he was treated by a citizen surgeon. January 21, 1865, he was mustered out of service.

Between nine and ten months after receiving the injury, and while there was still considerable discharge from the wound, and more or less soreness in the region of the sternum, he commenced working at his trade as a machinist. He soon found, however, that a little over-exertion or exposure to cold was certain to be followed by intense pain in the chest, difficulty of breathing, swelling of the tissues over the sternum, and a free discharge of pus; entirely disabling him for ten or twelve days.

He had suffered, as above described, three or four times each year since receiving the injury, and it was immediately after one of these attacks that he consulted me with the determination to submit to any treatment I might recommend for his relief.

Upon examination I found a small portion of the inner and lower margin of both lungs impervious to air. There was a sinus in the soft parts over the sternum, commencing a little below the upper end of the gladiolus, into which a probe could be passed downward nearly two inches toward the ensiform cartilage. Not until after slightly curving the instrument and turning the concavity toward the sternum could denuded bone be detected near the lower end of the sinus. After changing the angle of the probe a great number of times I succeeded in passing it entirely through the sternum against what I believed to be a foreign body; the passage through the bone being

obliquely downward and a trifle to the left side. The opening through the sternum being too small too admit the passage of an ordinary Nélaton probe, the patient was requested to call again the next day, when, after further examination, I would be prepared to give a decided opinion regarding his condition.

In the mean time I endeavored to make a substitute for the above-named probe.

With a fine saw a clay-pipe stem was quartered, the quarters being worked into probe shape. I then had a jeweller make a silver tube or ferrule four inches in length, of proper size to allow one of the clay probes to pass snugly into it, the probe protruding but a trifle beyond the end of the ferrule.

At the patient's second visit the sinus in the soft parts was laid open and the improvised probe passed through the opening in the sternum, and rotated against what we believed to be a bullet. Upon withdrawing the instrument it showed unmistakable marks of lead.

The patient was told that there was a ball beneath his breast bone, and that its removal was the only treatment offering any hope of relief. After explaining, as nearly as possible, the nature of the operation required for its removal and the dangers attending it, he determined to submit to the operation.

On the afternoon of October 15, 1870, assisted by Doctors O. P. Wolcott, E. W. Bartlett, and Wm. Thorndike, and in the presence of medical students, chloroform was administered, and a V-shaped flap dissected from the sternum, freely exposing the bone, the incision extending as low as the upper border of the seventh costal cartilage.

A five-eighths of an inch trephine was applied as nearly as possible over the bullet, which was in the centre of the sternum on a level with the costal cartilage of the fifth rib, and a disk of bone removed. We found, however, that the main part of the ball lay below the point where the trephine had been applied, and that only a ragged point of lead, protruding from its side and end, had been exposed. The trephine was again applied immediately below the opening already made in the bone, and a second disk removed. This brought into view all except the conical end of the ball, which was turned to the left side, and in order to bring it fully into view,

about one-third of a disk more of bone was removed from over that point.

The ball was very ragged, and inclosed in a thick strong cyst, excepting the point lying in close proximity to the opening through the sternum, and the whole very firmly imbedded in what seemed to be a fibro-cartilaginous mass, which we supposed to be areolar tissue changed to this condition by long-continued inflammatory action. The ball, which we afterwards found to weigh six drachms and twenty-one grains, was then removed, together with the cyst. The abnormal tissue, in which it was imbedded, held it with about as much firmness as bone, rendering its removal quite difficult.

At the bottom of the opening from which the ball had been extracted the pericardium could be distinctly seen pulsating, apparently considerably thickened at that part.

The dressing of the wound was delayed for some time, owing to the oozing of blood. After the cessation of the hemorrhage the V-shaped flap was brought down over the opening in the sternum and secured by sutures, excepting at the lower angle. A very thick covering of lint over the wound constituted the dressing. The anæsthetic acted kindly, and no unusual shock followed the operation. Opiates were prescribed to be given when necessary, to relieve severe pain.

From the notes of the case we extract the following:—

October 16th. Has not slept; had a chill during the night, followed by high fever; pulse 120; breathing hurried; pain and soreness through the chest; has coughed considerably during the latter part of the night; tissues over the sternum swollen and red. Prescribed ten drops of tinct. digitalis, with five drops of tinct. aconite root, to be given every six or eight hours. Opiates to be given when necessary to relieve pain. Fomentations applied over the sternum.

17th. Slept several hours under the influence of opiates. Cough troublesome; complains of soreness in the chest; breathing rapid; a discharge of pus and blood from the wound; pulse 120.

Treatment continued, with the addition of an enema to act on the bowels.

18th. Rest disturbed by cough; expectoration slightly tinged with blood; perspired profusely during the night; a distinct friction sound heard over the right side of the heart. Soreness and pain continue; pain less intense. A free discharge of thin pus, mixed with blood, from the wound; pulse 110.

From this time the symptoms were less grave from day to day, and the treatment was varied to meet the indications. The improvement, however, was exceedingly slow. The discharge from the wound continued unhealthy in character and quite profuse; the healing process being tedious. The cough and soreness also continued to trouble him for weeks. Was confined to his bed most of the time for a month after removal of the bullet.

Two months and a half after the operation he had sufficiently regained his strength to commence doing light work at his trade, notwithstanding the wound had not entirely healed, and there was still considerable soreness in the region of the sternum.

On January 22d, while making repairs upon a locomotive, with steam in the boiler, a stopcock was accidentally opened, allowing escape of steam, burning his hands quite severely. In order to avoid further injury, he jumped from the cab of the locomotive, striking heavily upon the floor of the round house. This effort caused what was thought to be an alarming hemorrhage from the partially healed wound; the bystanders declaring that he lost a very great quantity of blood. Almost immediately after cessation of the hemorrhage he found himself entirely free from cough, soreness, and pain, from which he had suffered more or less since first receiving the injury. Within ten days after the above-named hemorrhage the wound had completely healed, and has remained so from that day to this.

This case seems to us of interest, as adding one more to the list of those in which foreign bodies of considerable size have remained in close proximity to the heart for years, without fatal results, or even producing total disability. It also shows that the opening made through the sternum, by the passage of the ball, must have almost completely closed to have misled the surgeon who examined Captain B. at the time of and soon after the receipt of the wound.

In seeking for information concerning trephining of the sternum, we have examined quite a number of works on surgery, but find very little upon the subject, and no mention whatever is made of trephining for the removal of foreign bodies lodged beneath this bone.

Several authors, however, speak of trephining for removal of diseased portions of the sternum, and for the evacuation of matter from the mediastinum; also, formerly, for paracentesis of the pericardium. Mr. T. Holmes says: "This has the distinction of being one of the most ancient surgical operations . . . . . but it seems a doubtful and very hazardous means, and could only be justified by the presence of severe dyspnœa, for which no cause, except the disease of the sternum, can be discovered." He further says: "In Heyfelder's work (p. 308) will be found a notice of twelve cases in which this operation has been performed in modern times, and only one of which is said to have died from the operation. But real evidence of success seems wanting in the majority of these cases."

From the examination given the subject we conclude: That, had the operation and after-treatment in the case of Captain B. been conducted upon strictly antiseptic principles, the unpleasant and somewhat alarming symptoms following the operation might, to some extent, have been avoided. That, while trephining the sternum must, necessarily, be attended with some risk to life, it is not, with our present means of guarding against excessive inflammation, exceedingly hazardous; and is justifiable for the removal of foreign bodies lodged in the mediastinum, even though such bodies may cause only partial disability, as in the case reported.

DISCUSSION.

Dr. Alfred Post, of New York, N. Y.

I would like to ask the author of the paper whether he considered the rapid recovery of the patient due to the loss of blood which occurred at the time of the accident on the locomotive.

Dr. Marks. I did at the time. In reply to the question asked with regard to the removal of the sac in which the bullet was imbedded, I desire to say that it was impossible to extract the bullet without removing the sac. The bullet, as I have said, was roughened and the sac was firmly adherent to it.

FRACTURE OF THE NECK OF THE FEMUR.

By E. M. MOORE, M.D.,
OF ROCHESTER, NEW YORK.

[Read June 1, 1883.]

In this paper I do not propose an essay upon the fracture of the neck of the thigh-bone. With *such* an auditory this would be quite unnecessary. But knowing that you understand the general question quite as well as myself, I will take the liberty of calling your attention to a few points in rather a disconnected way, and ask a few questions upon them. Having become possessed of some specimens of this kind of fracture, very shortly after the death of the patients, which have produced some changes of opinion in my own mind, I submit them to your observation.

The causes of fracture of the neck of the femur are thus stated by Sir Astley Cooper:—

"In London, the most frequent cause of this accident is from a person, when walking on the edge of the elevated foot-path, slipping upon the carriage pavement, and though a distance of but a few inches, from its occurring so unexpectedly and so suddenly, it produces a fracture of the neck of the thigh-bone."

That he means us to understand that the break occurs from the direct blow, and not from any subsequent fall, is obvious enough from what follows below. Indeed, he regards this as the usual and common cause of the accident. For below, he says, "that a frequent cause of this accident is, however, a fall upon the trochanter."

It must be added that he says he has dwelt upon the subject of cause, for the young surgeon might not believe that one so trivial could result in an accident so serious.

Robert Smith alludes to Cooper's caution on this matter, and

narrates a case where a shop-woman broke the bone at the neck, in making a quick turn from the counter to a drawer, and did not fall at all; thus showing that the fracture resulted from muscular action alone. He also cites another case, resulting from the same cause, where the patient did not fall, the toe of the limb injured becoming entangled in a piece of carpet. But in every other of the long array of cases, where the cause of fracture is noted at all, the reason given is that of a fall upon the trochanter. I would, therefore, ask the question:—

Was Sir Astley Cooper probably correct in his observation with reference to the causes of fracture? Does it tally with that of any modern observer? For myself, I may say I have not seen a case of fractured neck of the femur, intra or extra-capsular, that did not come from a blow upon the trochanter, usually, of course, by a fall. I have known three cases of fractured thigh, in the middle of the shaft, from muscular contraction alone. But I should scarcely note these as proving muscular exertion to be the common cause of such fractures; or that it was other than very rare. My inquiries among my friends coincide with this, and many of Cooper's contemporaries never agreed with him. Robert Smith, however, believes with him, that the fall is often the result of the fracture, instead of the fracture resulting from the blow.

Sabatier, and other continental surgeons, regarded the blow on the trochanter as the cause of the fracture.

In the symptom of shortening, there is no difference of opinion among surgeons at the present day. Indeed, Smith states the case with clearness and caution, pointing out the difference between that which exists immediately after the accident, and that which increases after a few hours or days.

Cooper certainly states this symptom in an exaggerated manner, when he says: "The leg becomes from one to two inches shorter than the other; for the junction of the head of the bone with the cervix, being destroyed by fracture, the trochanter is drawn up by the muscles and carries with it the neck of the bone as high as the ligament will permit (meaning thereby the capsule), and, consequently, the trochanter rests upon the edge

of the acetabulum, and upon the ilium above it. This difference in the length of the limbs is best observed by desiring the patient to place himself in the recumbent posture on his back. When, by comparing the malleoli, it will be found that one leg is shorter than the other."

I quote this for an especial purpose, not to refute what no one at present believes, but to show that Cooper thought the reflected capsule, or, as Smith calls it, the cervical ligament, is entirely ruptured as the rule, and that the non-rupture of this band is the exception.

Smith recognizes the fact of slight shortening of the limb as the common condition immediately after the accident.

On this point there can be no controversy at present. He also attributes this to the non-rupture of the cervical ligament. That this is a power which restrains any considerable motion of the head at the point of fracture is a matter upon which there can be no difference of opinion. It also seems to me obvious that the non-rupture of the cervical ligament is almost universal, and that its rupture must be rare. If we could accept the doctrine that falls upon the feet were a common cause of the fracture we should see a reason for this rupture; but if the blow on the trochanter is the force that produces the result, I repeat, the rupture must be very rare.

If a patient should rise and attempt to walk complete rupture might occur; but such attempt is nearly always prevented by the severe pain which motion produces. As a matter of fact, such patients are ordinarily found on the bed to which they have been conveyed with a shortening of the limb so slight as to be inappreciable, up to that of half or three-quarters of an inch—seldom more. But in a few hours this is apt to increase.

The shock that the system has received passes away, and the pain will cause, by reflex action, a contraction of the muscles, so that the bands so important to the future condition, may be ruptured. This has been remarked by Robert Smith, but he did not follow the matter to its logical conclusion.

I think, however, this has been done by the profession which

has adopted the principle of extension, thus balancing the forces so as to retain the surfaces in contact.

Surely this is the condition which would, in any other fractured bone, result in ossific union, and in this place would be thus managed to produce a short ligament. How short such a ligament may be is illustrated by the specimen I now show you.

At its widest part it is not more than the eighth of an inch, and grows narrower even than this until it produces the appearance of bony union, though it would undoubtedly separate by maceration. This specimen has a scant history. A man called upon the late Dr. Howard, formerly Professor of Surgery in Starling Medical College, who made a diagnosis of non-fracture, and saw no more of the patient. About a year afterward a summons was served upon him to appear for trial for malpractice. This had no result, for death removed the plaintiff. Dr. Howard determined the diagnosis by procuring this specimen in due time. Allow me, at this point, to ask another question, and follow it with an expression of my own views.

What is the cervical ligament? Cooper, and he is followed by Hamilton and others, calls it the reflected capsule. This appears to me to be correct. It certainly has no analogue in the human body.

The capsule of a joint must naturally be derived from the edges of the articulating surfaces. This is so everywhere. I have examined the joints of inferior animals with reference to this matter.

In the turtle, where the acetabulum is shallow, though deeper than the glenoid cavity in the human subject, the capsule is derived directly from the edge of the acetabulum. But the great depth of the socket in warm-blooded animals would not permit motion with such an arrangement, hence, the insertion in the inter-trochanteric line. But nature works upon general plans, and here the law is manifest. During the early periods of fœtal life the membrane that invests the neck of the bone inside of the capsule is easily split, showing that the periosteum and the fibrous band, which is a continuation of the capsule, with its fibres

running longitudinally, are distinctly separate membranes. In the adult they coalesce so perfectly as to be one membrane.

Nevertheless, the strength of fibrous tissue is there, and when the fracture occurs the separated fragments are held by this tissue so as to produce the appearance of a ligament. Hence the name which has no anatomical rights, but which must remain in the surgical vocabulary as the cervical ligament, just as permanently and with quite as good reason as Poupart's or Gimbernat's. It must be classed as a rudimentary organ, so many of which have no use, and are, indeed, often a detriment to the organization.

In this case, however, the band is of incalculable benefit when the accident of intra-capsular fracture occurs. At first it has great strength. This is, however, gradually destroyed by the softening process of inflammation. I have known a case where the change of position in the femur, either by shortening of its length, or its eversion, as shown by position of the foot, was so slight, that a diagnosis of inflammation of the joint only was persisted in for three months. At the end of this time the change was rapid and extreme, the eversion of the foot was complete, and shortening of two inches at once appeared. This result could hardly have been attributed to any other cause than to the rupture of the ligament. Our modern treatment would, in my judgment, almost surely present such an occurrence.

The symptom of eversion of the toes in fracture of the femur, both intra and extra-capsular, is, in my judgment, a more persistent and reliable symptom, than even the shortening of the limb. I have nothing to say with reference to the rare cases of inversion, of which we have abundant record. But the extreme eversion which is shown by the foot lying flat on its outer border, when the patient is in the recumbent position, seldom occurs within the first few hours after the injury. Now, this slight eversion, which is the rule, is not overlooked by the experienced surgeon. The action of the rotators has been usually offered as the reason for this symptom, but this is obviously not the cause until there is complete rupture of the cervical ligament. The explanation offered by Prof. Bigelow, namely, that the tissue of the posterior portion of the neck is weaker than that of the

anterior, is clearly correct. Thus the blow on the trochanter crushes more of it at the weakest point, producing an eversion which is the same in the specimen denuded of soft parts, and in the living subject. This is very well demonstrated in the specimen I have to show you.

I have but little to say with reference to the extra-capsular fracture, but would call your attention to one or two statements of Robert Smith's; one is, that of a hundred specimens of this kind of fracture, he has never seen one that was not accompanied also with a fracture of the trochanter. This is singularly confirmed by this specimen that I now show you, in which the force was just sufficient to produce the fracture, and no more. There was no shortening, no eversion, no penetration. Still the fracture of the trochanter is begun. He also makes a statement, which I think all modern observers will confirm, that the extra-capsular fracture is usually accompanied, in course of time, by abundant osteophytes. This is illustrated by this specimen.

I will also exhibit another specimen, and with it the corresponding bone. They have been so beautifully prepared, that the cancellated structure is perfectly preserved, and this is reproduced and repaired so perfectly, that one would scarcely recognize a fracture at all. Yet the cervix is much shortened, showing that the impaction must have been considerable. Still, there is absolutely no redundant deposit. This was a patient of my own, seen immediately after the accident, the shortening was made out as it exists in the specimen. I may reasonably ask the question:—

Was not the freedom from redundant callus incident to the constant control of the muscles and other disturbing forces, while nature was carrying on the repair?

This case was that of a woman, seventy-two years of age, weighing two hundred pounds, who fell upon the trochanter while walking in the street. Both shortening and slight eversion of the toes were at once apparent, and the diagnosis was confirmed by crepitus. She recovered, always, however, walking lame. She died suddenly four years afterward.

Permit me now to call your attention to a collateral topic,

which, however, has a very interesting bearing upon the whole subject.

One of the most common, indeed, almost universal beliefs of the profession with reference to the cervix of the femur, is its liability to take on absorption, especially after injuries.

The cases of rheumatic arthritis that have been ascertained to present shortening of the neck of the femur are few, but well established by autopsy. There is uniformly great alteration in the shape of the head with a corresponding alteration of the acetabulum, and with restricted motions of the joint. Our best authority on this subject is Prof. Robert Smith. In all the specimens he has described in his classical treatise, the changes in the neck of the femur are accompanied with alterations of the head and its socket. The former becomes flattened, spread out as it were. Both it and the acetabulum seem to have been softened and fitted to each other. This is, undoubtedly, the fact, for the inflammatory process would produce such a condition.

"The cotyloid ligament," he says, "disappears, and the ligaments which complete the notch of the acetabulum are usually converted into bone. In the advanced stages of the disease, the ligamentum teres disappears, and the cartilage of incrustation is removed from both the head of the femur and the acetabulum. In several instances, the neck of the bone is surrounded by an intensely vascular fringe consisting of fimbriated productions of the synovial membrane. In such cases, the corona of the head of the bone is absorbed at different points, and the excavations are filled by these vascular fimbriæ. The acetabulum undergoes various alterations of form and size; in some cases, without becoming deeper than natural, it is found increased to more than double the normal size, measuring from ten to sixteen inches in circumference; in such cases, it is usually of an oval shape; sometimes it forms a deep and circular socket, while at the same time its margin is much contracted, and embraces the neck of the bone so closely that the head of the femur cannot be disarticulated. The head of the bone loses its spherical form."

I will not employ your time in quoting at length from his beautiful description of the various odd forms that the head

assumes under the pressure that it receives from the weight of the body. The relation of this joint to the forces that it has to resist, although sufficient in the normal state for all the demands made upon it, seems to be different from that of any other part of the body, when affected by disease. It has to bear all the pressure that can be brought to act upon the knee, and it is surrounded by much more powerful muscles. And yet, its far inferior size renders it less able to resist these forces. We are made painfully aware of this, when treating hip-joint disease in the young. We can very well understand that inflammation of the bones, constituting the joint, if of a low grade, would cause an alteration of the whole apparatus under the crushing weight of the body, and the powerful muscles which control the movements of the hip.

The knowledge of these facts has produced the induction that has been recognized by the profession, namely: That, after blows upon the trochanter, there are cases in which the absorption of the neck will take place following the inflammatory process. But I think it is inconceivable that this process should be confined to a portion of the neck of the bone. Indeed, if the blow produced no fracture, it is obvious that the joint surfaces and their subjacent osseous tissue would be the primary seat of change.

Recurring to the induction that has been tacitly admitted I would ask the question:—

Is it a reasonable, much less a perfect induction to assume a similar cause, when the changes of condition are similar only in one point, and dissimilar in every other?

Now, many of the cases that have been brought forward as examples of bony union of the neck in intra-capsular fractures, following blows upon the trochanter, do not have alterations of the head, either in its form or surface, do not have changes in the acetabulum, do not have alterations of the synovial membrane and cartilage with its worm-eaten surface. On the contrary, the joint plays with ease, only circumscribed in the swing of the femur, by the shortened neck being brought in contact with the rim of the acetabulum, perfectly able to bear the weight

of the body without the distress and the soreness incident to the inflammatory process.

I might ask the question, May not the induction be properly stated thus, and with more philosophical correctness?

The neck of the bone is shortened after the blow, but it cannot be from absorption incident to inflammation, for the evidences of this process are totally absent.

To illustrate this principle, I exhibit to you now a specimen which, I think, possesses very peculiar and instructive conditions. The history of the case was not very clear at the time of injury. The patient, seventy-eight years of age at the time of his death (which occurred during last winter), entered the Buffalo General Hospital November 21, 1871. He reported his lameness as occurring after a fall upon his hip, and that he was unable to move about for several months, even with the aid of crutches. But the case was complicated by the existence of rheumatism. I have here the superior extremities of both thigh-bones, and the acetabulum corresponding with the injured one. You will observe how admirably it illustrates the description just given of these cases. The head is certainly twice the size of its fellow. It is also flattened and worm-eaten in appearance, with a shortened and thickened neck. The acetabulum is quite as interesting in the pathological changes; the edge is elevated, and irregular, obviously moulded upon the altered head.

Among the valuable contributions to the elucidation of the surgical injury under consideration, I must not omit to mention that of Dr. George Smith, who has made an elaborate study of the capsular ligament in sixty-one cases. His tables show a great variation of the point of insertion of the capsule on the neck of the femur. This he has carefully demonstrated by injecting the joint cavity with plaster of Paris, thus proving the fact of variation beyond the reach of cavil. Such variations were to be expected; they correspond with the results from similar close observation in every other anatomical condition. But the whole tone of his elaborate criticism with reference to the possibility of union by bone of intra-capsular fracture, is to show the impossibility of making a diagnosis with reference to

the important point, that by common consent governs the whole question, viz., the impossibility of ascertaining if the line of fracture is entirely within the capsule. He makes a criticism of the cases of reputed union, that they always show absorption at the neck, with the corollary, that this absorption destroys the ability of knowing whether the diagnosis be correct or not, assuming that if the absorption is carried beyond the line of the capsular insertion, even if the fracture was not so at first, it must be considered as properly coming under the category of extra-capsular rather than intra-capsular.

On page 14 of his pamphlet on this subject, he criticizes Professor Robert Smith's conclusions by saying: "Definite measurements of this specimen are not given in the description of the post-mortem appearances in Mr. Smith's work; consequently, we have no means of knowing the exact location of the line of union. The history states that the neck was shortened, but does not tell us how much. In most cases of union after fracture of the neck, it is shown that union did not occur till after the posterior surface had been nearly or quite removed by absorption. All of Prof. Mussey's cases of union are examples of this kind. Prof. Parker's has suffered the same loss. Mr. Adams's case is another illustration of this change. If a great portion of the posterior surface of the neck be removed before union takes place, the line of union will be found without the normal capsule; and, in these cases, it is impossible to tell at what point the fracture occurred." Again, on page 18, he says, after speaking of several cases brought forward as evidences of bony union: "If absorption did follow the fracture in each of these cases to the complete removal of the neck, the impaction of the fragments, which Mr. Smith considers so indispensable to bony union of this fracture, must have been loosened, so that in accordance with his own theory, he could not have hoped for bony consolidation. Certainly, the force of the blow which produced the fracture was not sufficient to crush and obliterate the neck. The sections of these specimens show that the neck has neither been driven into the shaft, nor the head of the bone; yet the neck is gone, and the head is united to the shaft. I do

not see how these specimens can be regarded as examples of impacted fracture; neither do I see how it can be known that the fracture in either case was within the normal capsule; but if the fracture in each case were intra-capsular, these specimens furnish strong encouragement for us to hope for bony union under favorable circumstances, if the fragments be approximated, even after the entire removal of the neck by absorption."

I have made this long quotation from Dr. Smith's elaborate essay, which is certainly a strong polemic upon the side of belief in constant non-union of the neck by bone. And, as you will perceive, he lays stress upon the fact that the blow would not crush and obliterate the neck to the extent exhibited in the cases brought forward as evidences of bony union. Compare these opinions with the facts in the three cases I will report to you.

I do not propose to enter into a protracted argument upon this point. I believe there is an important fact that has been over-looked with reference to the appearance of absorption of the neck; a fact that has, in my own mind, made a complete change in my antecedent impressions on this matter. I will now relate some of my cases to show that absorption is a small element in the shortening of the neck, instead of being its sole cause. I may add, that in looking over the twenty-eight cases of intra-capsular fracture in Robert Smith's tables, that death has occurred seven days after the accident in one case, ten days afterward in two cases, and fourteen days afterward in three cases. They are all sunken down, and the shortening of half an inch in the length of the femur is recorded of each one.

Case I.—During the winter of 1858 Mrs. S., aged sixty, fell over on the trochanter while walking on a broad stone pavement. She was a slender woman, and made no attempt at walking after the injury, and was carried a short distance to her house. Upon measurement from the anterior superior spinous process of the ilium to the malleolus a shortening of half an inch was apparent. This was accompanied with slight eversion of the foot. Great pain persisted until the eighth day, when she suddenly died. Upon removing the upper end of the thigh-bone from its socket the neck was found to be broken inside of the capsule. The cervical liga-

ment was found intact at the inferior part as I now exhibit it to you. Far more, however, was present than is to be now seen; for it has been somewhat destroyed by much handling. My first surprise that so much of the neck could have disappeared by the process of absorption in so short a time did not at that time, however, suggest any other explanation.

Case II.—F. H., an unmarried woman of seventy-two years of age, and weighing about two hundred pounds, fell upon the trochanter while walking upon a broad flagged pavement. She was removed to her home in a most helpless condition, and undergoing great suffering, which continued until her death. She was placed in bed, obviously much shocked, and died in three days to an hour from the time of the accident. At no time could we convince ourselves that there was any shortening of the limb.

Repeated measurements from the ilium to the malleolus and the knee resulted in the same conviction. Neither could crepitus be obtained, but the nature of the blow, the great pain, with slight eversion of the toes, rendered the diagnosis easy. I had always felt that our knowledge of the amount of shortening of the neck in the case of Mrs. S., which I have just shown you, could not be accurately known without being able to compare it with its uninjured fellow. I therefore removed both bones when making this autopsy. The value of this method is apparent. This specimen has completely dispelled from my mind the idea that absorption plays any considerable part in the semi-sessile position of the head in the cases supposed to be intra-capsular, and yet united by bone. From the nature of the circumstances there could have been no absorption.

The time was too short, and the patient was shocked unto death, and this active process could not have been established. We have in this case an illustration of the condition at the moment of injury.

I would call your attention to the cause of the absence of shortening in the length of the femur, notwithstanding the great abbreviation of the neck. The explanation that has been offered by Prof. Robert Smith, that the absence of shortening was due to non-rupture of the cervical ligament is not sufficient. The head is slightly tilted up. This, it appears to me, can be easily explained, if the person in falling over would have some mo-

mentum that would carry the body upward at the moment of contact. We know, as a matter of fact, that there is in most cases a shortening from a quarter to three-quarters of an inch. This can be easily understood, for in most cases there would be extreme tension of the muscles.

The reasons that are given for non-union of the neck are various, as you all know. They are the motion of the parts upon each other, their separation by muscular action, the absence of vitality in the head from its diminished supply of blood (regarding this as only coming from the round ligament), the excess of synovial fluid bathing the parts, diluting and altering the exudate. All these we hear of, and perhaps the most stress is laid on the deficiency of blood, showing the general impression that there is a complete separation of the cervical ligament as the rule. I do not intend to follow out these points, as I have asserted that I would not write an essay, but would only ask a few questions. At this juncture I may inquire, Is there then the slightest reason to believe there is any lack of blood necessary to ossific union?

I assume that the average cases are like those I have shown you. The periosteum is largely preserved. The very traumatism would induce increased vascularity. The loss of periosteal connection in any other part of the body is followed by caries or necrosis, as the rule; here we may say almost never. I will offer the existence of the crushed surface as the chief obstruction to the bony result. In the multiplicity of opinions, this may have been offered, but I have not happened to learn it. The surface is made up of displaced fragments, which would seem to be liable to caries. This, of course, we know, does not occur, but a certain amount of inflammation would take place, unfavorable to physiological repair.

The use of the word impacted has been a little indefinite. It is commonly employed when speaking of extra-capsular fractures, and as indicating their condition. The compact tissue of the lower portion of the neck breaks its way into the shaft and becomes impacted in the cancellated tissue of the femur, as illustrated by this specimen, a mere dissecting-room waif, but one

that tells its own story. There are, however, rare cases of penetration of the head by oblique fracture of the neck, when the sharp compact tissue is fairly driven into the cancellated tissue of the head. These are also regarded as impacted. In the cases I have exhibited of recent intra-capsular fracture, there is no impaction in either of the senses described above. The surfaces are free from any osseous entanglement. The tissue is crushed upon both sides, but not held together from this circumstance. By this change, produced by crushing, you have an extreme shortening of the neck. The cases of reputed bony union are scarcely more shortened than this one taken from Case I., and not much more than the other one from Case II.; and yet in both there are perfect intra-capsular fractures. The argument, then, that absorption of the neck is fatal to the belief in the possibility of ossific union falls to the ground at once, for it is not absorbed. I do not mean to say that absorption of the neck does not occur. When there is no union, it almost disappears. Nature leaves no ragged edges. They are taken away, and the ultimate condition is that of complete absorption. She does not keep on hand useless material, but if the parts held together by the cervical ligament, aided by the weight that counteracts muscular strain, and drawn together by the action of the adductors, should unite by bone, there would be no need of absorption. Nature would retain the whole, and the whole would be well represented by the length, shape, and position of these recent specimens, in which we know the neck is not absorbed, but changed by displacement of tissue. But while these surfaces are in no sense impacted, they are capable of neat apposition. The wavy irregularity is favorable to the contact so necessary to bony formation. This very condition prevents the production of crepitus, causing the head to follow most motions of the shaft, and will also prevent the separation of the fragments upon motion of the patient, provided there is a proper adjustment of the counteracting forces.

I think nearly all of us have adopted a uniform plan of treatment during the last few years, which has, perhaps, grown into use from the employment of gravity in the treatment of fractures

of the shaft. Sir Astley Cooper's instructions have certainly controlled the profession until within a short period of time. I have, however, abandoned his views of treatment in every case for the past twenty years. My custom is to append a weight to the limb in all cases, without reference to any diagnosis of an intra- or an extra-capsular condition; but I offer no restraint by side-splint or even sand-bags. I do not interfere with the change of angle of the body by increasing or diminishing the pressure. My purpose is to maintain the amount of tension which just balances the traction of the muscles.

This is, of course, a variable quantity, and I have learned to allow the patient to be the judge of the proper weight, increasing or diminishing it at his or her pleasure. The sensations of the sufferer are the best guide. If there is too much weight, the tendency must be in the intra-capsular fracture to separate the fragments and place strain upon the cervical ligament. If there is not weight enough, the muscles will produce similar effects in the opposite direction. Without aiming at bony union we are almost sure to preserve the integrity of the periosteum in the cervical ligament.

This must furnish the material of repair, and likely to result at least in a short and strong ligament adequate to the support of the body. If we abandon the attempt at retention, through despair of union, the cases would soon justify Cooper's delineation of symptoms: namely, extreme eversion of the toes and a shortening of one or two inches. Indeed, the sudden rupture of the ligament will produce all this, as I have elsewhere said. The treatment of the extra-capsular form is precisely the same as I have before stated. In these cases no great alteration of condition can be expected from the weight appended to the limb; but it will counteract the strain of muscles tending to shorten it. A plan of this kind renders the necessity of diagnosis a matter of small consequence, except as regards the prognosis. Even here my experience convinces me that there will not be the difference in the practical result in walking that formerly existed between the two kinds of fracture. A short ligament will sustain the body, and such confidence in producing this result has grown in my own mind,

that I do not hesitate to hazard a prognosis of this kind: "You will always be lame; the limb will be shorter than its fellow from one-half inch to an inch; your step will be shortened upon the affected side." This result is so uniform in consequence of the method by extension, that this treatment should, in my opinion, never be omitted. The fear of breaking down the powers of life by the confinement to the bed appears to me to be an error. On the contrary, the relief obtained more than compensates for the injury of confinement, and activity is not the rule in the later periods of life.

In conclusion I submit the following questions as embracing the points which I desire to develop in the preceding remarks:—

1. Is not the cause of fracture of the neck of the thigh-bone, whether intra- or extra-capsular, almost uniformly that of a blow upon the trochanter?

2. Is not the preservation of the periosteum of the neck, called, in connection with the reflected capsule, the cervical ligament, although only partial, the common rule and not the exception?

3. Does not this cervical ligament, if preserved, supply abundant nutriment to the upper fragment sufficient to entire repair?

4. Is not the outer layer of what is called the periosteum of the neck, a rudimentary organ?

5. In *reputed cases* of absorption of the neck after blows upon the trochanter, said to be without fracture, is it a reasonable, much less a perfect, induction, to infer a similar result, when the changes of condition are similar only in one point, and dissimilar in every other, from those cases of inflammation without a blow?

6. Should not the induction read thus: The head of the femur and the acetabulum, not being altered, the shortening of the neck could not be from the inflammation resulting from the blow?

7. Finally, does not the practice of modern surgeons produce a vastly improved result in the cases treated by them, as compared with the methods of the last generation?

DISCUSSION.

Dr. R. B. Bontecou, of Troy, New York.

Am I correct in understanding Dr. Moore to say that fracture of the neck of the femur is always produced by a fall?

Dr. Moore. All the cases I have seen were produced by a fall; but cases have been reported where the individual did not have a fall.

Dr. Bontecou. I have made post-mortem examinations in two cases in which the injury was caused by a simple twisting of the limb while the foot was resting on the floor. In these cases the examination revealed very slight attempt at repair six weeks after the receipt of the injury, but the singular manner in which the injury was induced made it rather remarkable.

Dr. Alfred Post, of New York, N. Y.

Dr. Moore has selected a class of cases of intra-capsular fracture of the neck of the femur where there was no appreciable shortening, and it seems to me that there is a very important practical suggestion to be made in regard to that class of cases; and that is, not to make any meddlesome investigation in order to ascertain the diagnosis. I remember a case of that kind which came under my care in the New York Hospital, in which there was reason to suspect the existence of a fracture of the neck, but where we could not make out any shortening of the limb. There was very marked lameness and some degree of eversion of the foot, but there was an absence of certainty as to the diagnosis of fracture. It was toward the close of my term of service in the Hospital. I believed that there was a fracture, but thought it important not to verify the diagnosis by making any extensive motion. My successor, who came in charge a few days after, was not satisfied with the uncertain diagnosis, and made some extensive motion of the limb in all directions; and after such movements had been made there could be no doubt of the existence of a fracture. The probability is that there had been some dove-tailing or impaction which held the fragments in position, and which, if it had not been disturbed, would have secured bony, or at least ligamentous, union. The examination made by

my successor produced a very decided fracture, and the result was non-union. I have been very careful in such cases to treat them as fractures of the femur, always allowing some uncertainty of diagnosis, but giving the patient the benefit of the doubt. I do not think we have the right to use means for making a certain diagnosis to the disadvantage of the patient. I think that the rule to give the patient the benefit of the doubt in all cases is the best. With regard to the statement of Dr. Moore that the shortening of the neck of the femur in intra-capsular fractures is the result of crushing down the spongy substance, and not of interstitial absorption, I think there is an argument opposed to that view of the case, namely, that in recent cases the amount of shortening is generally inconsiderable (*i. e.*, is less than an inch). If there be a primary shortening exceeding an inch, it is very good evidence that the fracture is extra-capsular. But in those cases where the individual has lived for months or years, there is marked shortening. It appears to me that this indicates that in such cases the shortening is the result of interstitial absorption.

Dr. Moses Gunn, of Chicago, Illinois.

I think I can recall to the Association a circumstance which occurred in the Surgical Section of the American Medical Association, at the meeting in Detroit, a few years ago. During the discussion that followed the paper of Dr. Sayre, a gentleman who was present as a delegate to the Association, asked that a committee might be appointed to take a measurement of his limb. The history of his case I was conversant with, for I was called to see him at the time of the accident. He was a man fifty years of age, rather heavy. Having fallen from the piazza of his house, he struck with considerable force. I saw him perhaps ten hours after the accident, and became convinced not only that he had a fracture, but that it was located far out on the neck, as being demonstrated very easily by the relative rotatory motion of the two sides. On the right side the rotation caused the formation of the arc of a circle, which was complete. On the left side it formed the arc of an unappreciable circle, or no circle at all. There was the usual shortening and the usual eversion; in fact, it was a typical case of fracture, a fracture located high up. I did not see the case again. The patient was attended by some of the neighboring physicians; his own judgment playing an important part in the treatment of the case. He recovered with apparently

bony union, and shortening of only about a half inch. He walked latterly without the aid of a cane, and was very proud of the result of the treatment in his own case. Perhaps it was five or six years later that he appeared on the floor of the Surgical Section, asking that a committee be appointed to take the measurement of his limb. The committee examined, and the result was that they found unmistakable shortening of two or two-and-a-half inches. Here was a time which marked the incomplete fracture, and a time five or six years later, where there was a shortening of two or more inches, the result unmistakably of interstitial absorption. I think it bears very strongly on the subject of shortening in fractures generally. I think the Fellows, if they make the attempt, can recall cases in which they have seen similar shortening.

Dr. A. VANDERVEER, of Albany, New York.

I wish to call attention to a specimen given to me by Dr. Beech, living in a neighboring town. He came to me one day and told me that a lady was about to die, whom he had treated several years ago for what he thought was an intra-capsular fracture of the neck of the femur, and that he thought he could get the specimen if I would like to have it. She did die, and we procured the specimen. The Doctor then proceeded to clean the specimen, but as he was in a hurry, he asked his office boy to attend to it. The result was that he boiled it for too long a time, and we found that instead of the bony union we expected to find, we had one of the most characteristic specimens of ligamentous union I ever saw. The patient's age was about sixty-five years. We have in our museum several specimens which were given by the late Dr. March. There was, in 1865, a very spirited discussion between him and Dr. Martin. Dr. March thought he had several specimens of bony union, but they boiled all the specimens, and in every case the fragments separated, showing that bony union did not exist. I may say that the results were good, when the time of life, at which the injuries occurred, was taken into consideration, the patients being able to get about after the employment of the simple treatment Dr. Moore has called our attention to, and which others have followed out. I think it is a very valuable treatment.

FRACTURES OF THE NECK OF THE FEMUR,

With Special Reference to Bony Union after Intra-capsular Fracture.

By N. SENN, M.D.,
SURGEON TO THE MILWAUKEE HOSPITAL, MILWAUKEE, WISCONSIN.

[Read June 1, 1883.]

I. *Experiments on Animals.*

These experiments were undertaken with a view of obtaining information concerning the following questions: 1st. What is the mode of repair after non-impacted intra-capsular fracture of the neck of the femur? 2d. What becomes of a bone or metallic nail when driven into the neck of the femur and retained permanently? 3d. What is the effect of such a nail upon the adjacent bone tissue? 4th. Can we, in cases of intra-capsular fractures of the neck of the femur, by immediate or direct measures, as by nailing the fragments together, obtain such accurate coaptation and retention as to secure union by bone?

A great many difficulties were encountered in performing these experiments, prominent among them I will enumerate shortness of the femoral neck, difficulty in carrying out the antiseptic treatment, and in providing additional means for securing immobility of the fractured bone, and the great danger to life in using anæsthetics. After I had lost a number of animals from the administration of chloroform and sulphuric ether I relied exclusively on hypodermic injections of morphia in preventing pain during the operation. The experiments were made on cats, dogs, and rabbits, embracing in all thirty-three operations upon thirty animals. In the first thirteen operations the capsule of the hip-joint was exposed by a small posterior

incision, and the neck was rendered more accessible by forcibly rotating the thigh inward; the bone was perforated a sufficient number of times with a small drill close to the head, and usually fractured by forcible abduction and rotation outward of the limb. The fracture usually took place with a distinct snap, and was followed by all the characteristic symptoms of fracture through the neck; preternatural mobility, shortening, and crepitus. The incision was closed with catgut sutures, and the wound covered with iodoform and salicylated cotton. In all of these cases the fractured bone was replaced as nearly as possible in the normal position, and a plaster-of-Paris dressing applied, which included the pelvis and both extremities. Two of these animals died of pyæmia, and in not a single instance out of the whole number could be found the slightest attempt at bony union at the post-mortem examination. In one instance (a young Newfoundland dog) the hip-joint presented evidences of severe inflammation without suppuration; the head of the femur, having necrosed, was found completely detached in the acetabulum. In some cases ligamentous union had taken place, while in others the fractured surfaces were covered with healthy granulations. In all the specimens the lower fragment had become shortened. Having satisfied myself that the antiseptic treatment could not be followed with sufficient accuracy in these cases to protect the animals against infection, I determined to fracture the neck subcutaneously. In the next six cases, after shaving and disinfecting the hip, and rotating the thigh inward and sliding the skin forward, I made a puncture down to the neck of the femur from behind with a narrow tenotome, and inserting the drill into the passage made, divided and fractured the neck as before. The skin retracting made the operation entirely subcutaneous. A plaster-of-Paris dressing was applied in the same manner as in the first series of experiments. No inflammation or febrile reaction followed these operations, and the post-mortem examinations showed evidence of ligamentous repair. In the absence of bony union, the functional result in several cases appeared remarkable. With few exceptions, all of the fractures produced

so far were proved to be purely intra-capsular at the post-mortem examinations.

In Experiment No. 21 the neck was fractured subcutaneously and no retaining dressing applied. The animal was killed five weeks after the operation, and an examination of the hip-joint showed that a firm and short ligamentous union had taken place. After the first three weeks little or no lameness could be detected.

Having failed in all cases so far in obtaining union by bone I determined to secure immediate and direct coaptation by nailing the fragments together.

The fracture was produced subcutaneously in the same way as in the preceding series of cases, and, after replacing the limb in its natural position and sliding the opening in the skin to a point corresponding with the centre of the base of the femoral neck, the drill was introduced, and a perforation made in the direction of the centre of the femoral head, and a wire-nail or bone-peg of proper length was driven into the opening made by the drill so that the outer extremity of the nail should not project beyond the surface of the bone.

The first two animals progressed very favorably after the operation and appeared to suffer but little pain, but unfortunately escaped before an examination could be made to ascertain the result.

Experiment No. 24.—Young cat; fractured the right femoral neck subcutaneously, and nailed the fragments with a bone-nail. Animal killed ten weeks after operation. Neck of femur almost entirely absorbed; capsular ligament thickened; vertical section through head, neck, and upper portion of shaft shows that the head is almost in contact with the trochanteric portion of the femur; posterior portion of neck shows line of fracture near the head, and fractured surfaces in close contact, but movable upon each other; anterior portion firmly united by a dense compact callus, the upper fragment apparently impacted into the lower; no trace of the bone-peg can be found. The perforation in the trochanter major can be followed to a distance of about two mm. In this specimen the lower fragment appears to have become almost completely absorbed, as the upper fragment remains unchanged, and appears to be almost

in direct contact with the trochanteric portion of the femur. Ligamentum teres normal.

Experiment No. 25.—Adult cat; subcutaneous fracture of neck of right femur; direct transfixion of fragments with wire-nail. Animal killed eighteen weeks after operation. Fracture within capsule close to the head; fragments in close contact, slightly movable upon each other, but united by a very short ligament. Nail had slipped outward, and projected from the trochanteric surface about one-third of an inch, and could be felt as a sharp point immediately under the skin. The projecting portion of the nail is invested by a firm, dense, fibrous capsule, while the implanted portion is firmly and immovably fixed in the bone. Vertical section through the head, neck, and trochanteric portion shows that almost the entire neck has disappeared by interstitial absorption, the upper fragment being almost in contact with the trochanteric portion. The trochanteric portion has almost entirely lost its cancellated structure, its interior being filled with compact tissue; this change is conspicuous more particularly in that portion traversed by the nail. Capsular ligament thickened; ligamentum teres normal.

Experiment No. 26.—Adult, large Maltese cat; subcutaneous fracture of right femoral neck; direct coaptation of fragments with wire-nail. Animal killed ten weeks after operation. Neck of femur shortened; capsular ligament thickened; ligamentum teres normal; vertical section through the upper portion of the femur shows line of fracture within capsule, with impaction of upper fragment into lower; fragments movable upon each other, but broken surfaces in immediate contact. A new compact layer of bone was formed on the outer surface of the compacta in the region of the lesser trochanter. Nail firmly imbedded in bone, outer extremity on a level with compact layer of trochanter major; it is seen to traverse the trochanteric portion in a backward direction, entering the cavity of the hip-joint, and being in close contact with the posterior surface of the femoral neck; its sharp point being on a level with the highest point of the head. No inflammation in the hip-joint. During life the function of the joint appeared to be perfect. As the point of the nail was firmly fixed in the capsular ligament, and impaction had taken place during the nailing process, immobility was tolerably

well attained, and there is every reason to believe that bony union would ultimately have taken place.

Experiment No. 27.—Adult Maltese cat; subcutaneous fracture of left femoral neck; fixation of fragments by means of bone-peg made from compacta of tibia of an ox. Animal killed fourteen weeks after operation. Neck of femur only slightly shortened; capsular ligament nearly normal; ligamentum teres normal; vertical section shows a slight curve in the upper portion of the neck, the head being slightly depressed. Perfect and complete bony union, the spongiosa being restored nearly to its normal condition. Bone-peg has disappeared completely.

Experiment No. 28.—Old Maltese cat; subcutaneous fracture of left femoral neck; direct adjustment of fragments by bone-peg. Cat died of fatty degeneration of liver and kidneys five weeks after operation. Vertical section through upper portion of femur reveals line of fracture partly within and partly without the capsule; no union; fragments in good apposition; outer extremity of bone-nail beneath the compacta, direction of nail downward and inward, the point terminating a little beyond the line of fracture in the lower portion of the neck. The saw has divided the nail obliquely at the juncture of the outer with the middle third. No evidences of inflammation or repair.

Experiment No. 29.—Adult cat; fractured neck of left femur subcutaneously, and used bone-peg for nailing fragments together. Animal died of pyæmia twelve days after operation. Hip-joint filled with pus; fracture intra-capsular; outer extremity of nail on a level with compacta, its point was in the cavity of the joint on a level with the foveola of the head. A piece of the posterior portion of the head is split off, an accident which occurred either by the drill or driving in of the nail.

Experiment No. 30.—Adult cat; subcutaneous fracture of right femoral neck, and direct transfixion of fragments by wire-nail. Animal died, four weeks after operation, of pneumonia. No inflammation of joint; fracture intra-capsular; fragments slightly separated but well transfixed by nail; no callus.

Experiment No. 32.—Young cat; subcutaneous fracture of neck of right femur; direct fixation of fragments with bone-peg. Animal killed four months after operation. During life function of the joint was perfect; vertical section through the head, neck, and upper portion of shaft, shows that the line of fracture must have been entirely within the capsule, as no thickening of bone or ligament could be seen; capsular ligament normal. Accurate measurement shows only an appreciable shortening of neck; compact tissue within neck more abundant than in the opposite bone. Spongiosa restored to nearly its natural perfection. No trace of track of perforation or bone-nail.

In no case did I feel crepitation more perfectly than in this case, and the sudden giving way of the bone the moment it was fractured was well marked, and heard by several witnesses, and as the post-mortem examination shows a most perfect restoration of the continuity of the bone, I am convinced that this case represents a typical and perfect recovery of union by bone after intra-capsular fracture of the neck of the femur.

In all cases, twenty-one in number, where no direct means of fixation were used, there was not the slightest evidence of bony union, the best result attained was a short ligamentous band. In Experiment No. 21, no retention dressing was applied, and the result was equally good, if not better, than in the cases where the plaster-of-Paris dressing was used.

In all of these cases the tendency to shortening was not as well marked as in man, while eversion occurred seldom, and only to a slight degree.

The weight of the limb evidently counteracted muscular action, while the conditions which produce eversion in man are absent in animals. The results obtained by immediate transfixion of the fragments stand in direct contrast to those treated by external fixation. Bony union, or union by short ligament, was the rule, non-union the exception.

These experiments would also tend to prove that aseptic metallic nails, when implanted subcutaneously into living bone, remain firmly in its substance for an indefinite period of time without giving rise to suppuration, and from one of the experi-

ments it will be seen that the point of the nail was within the cavity of the joint for many weeks without materially interfering with the normal function of the joint, or producing more than a slight synovitis.

Iron- and bone-nails, if driven into living bone, produce osteoplastic inflammation, and are, on this account, not only useful in the treatment of pseud-arthrosis, but are equally beneficial in accelerating the reparative process in recent fractures. Bone-nails are completely absorbed, the time required for absorption to take place depends upon the vascularity of the tissues, which are in immediate contact with the nail.

According to Gurlt, the time required for bony union to take place is proportionate to the diameter of the fractured bone, being much shorter in case of slender bones as compared with those of greater diameter.

It appears that the shortest time for the slender neck of the femur in cats to unite by bone is at least two months; hence in man the time required for bony consolidation of fracture of the femoral neck must be at least from one hundred to one hundred and twenty days. As in two of the specimens, well-marked impaction occurred during the nailing process, the question arises if the same desirable conditions could not be obtained in man by using sufficient lateral force at the time direct coaptation is attempted; in other words, would it not be prudent to use sufficient force to produce artificial impaction.

In nearly all the specimens the upper fragment underwent but little change, while the lower fragment always, without exception, suffered a diminution in length from osteo-porotic inflammation and interstitial absorption.

Interstitial absorption, as the consequence of inflammatory osteo-porosis, takes place to a greater or less extent in every case of fracture through the femoral neck, and precedes and accompanies the reparative process. In all cases of bony union the posterior attachment of the cervical portion of capsular ligament was displaced outward, an occurrence which can only be explained satisfactorily by assuming that during the osteo-porotic inflammation, the periosteal investment of the femoral neck is

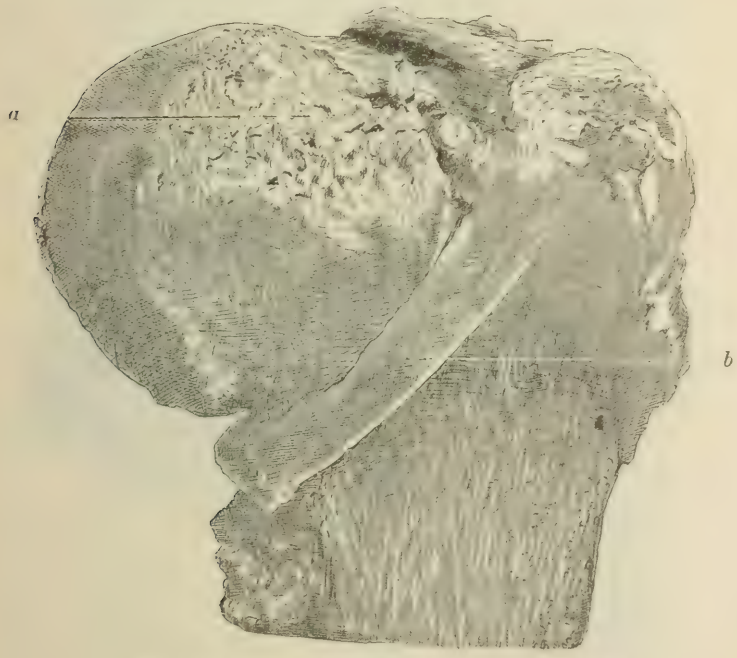
loosened and transplanted toward the femoral shaft, carrying with it the femoral insertion of the capsular ligament. These experiments also illustrate the difficulty of transfixing the upper fragment in the process of nailing, a circumstance largely due to the diminutive size of the bone, the incomplete anæsthesia, and the want of fixation of the parts in their relative normal positions previous to the operation.

II. *A Case of Bony Union after Impacted Intra-capsular Fracture of the Neck of the Femur.*

At our last meeting I presented a specimen of bony union after an intra-capsular fracture of the neck of the femur, and gave a full description of the case and specimen (see page 167). In the discussion which followed, it became not only evident that in the opinion of the speakers the specimen was not what I had claimed for it, but that such a favorable occurrence was not possible, and that a well-authenticated case had never been observed. These criticisms induced me to look up the literature on the subject with care and impartiality, and to resort to experiment to verify my position. I will ask the Fellows to examine the specimen once more, and will call their attention to a few prominent points in the case. The patient was a female, aged seventy-five years, and was under my observation at the Milwaukee Hospital. She was in good health at the time of the accident, hence there can be no possibility that the extensive changes in the neck of the femur were the result of senile coxitis or interstitial absorption. The fracture was produced by direct violence by a fall upon the greater trochanter. Fractures of the neck produced in this manner are very apt to be impacted. Loss of function was complete immediately after the injury, and remained so for several months. The patient suffered great pain in the groin and the region of the trochanter minor, a symptom which is always indicative of injury within the capsular ligament. For the purpose of excluding asymmetry of the bones, all the long bones of both legs were measured separately, and on comparing the measurements

the injured limb was found shortened one-half of an inch. The limb was strongly everted. Gentle traction had no effect on the length of the limb. On comparing the movements of the trochanter major on both sides by rotating the limbs it was found that the neck of the femur on the affected side was perceptibly shorter. No crepitation could be felt. As the impaction appeared to be firm no treatment was employed, except rest in bed on a smooth even mattress, and the limb supported on each side with sand-bags. In this position the patient remained for three months; at the expiration of this time she was allowed to walk on crutches. After the third week the shortening gradually increased until it reached an inch and a half, when the treatment was suspended. The secondary shortening I attributed to inflammatory osteo-porosis and interstitial absorption in the lower

Fig. 1.



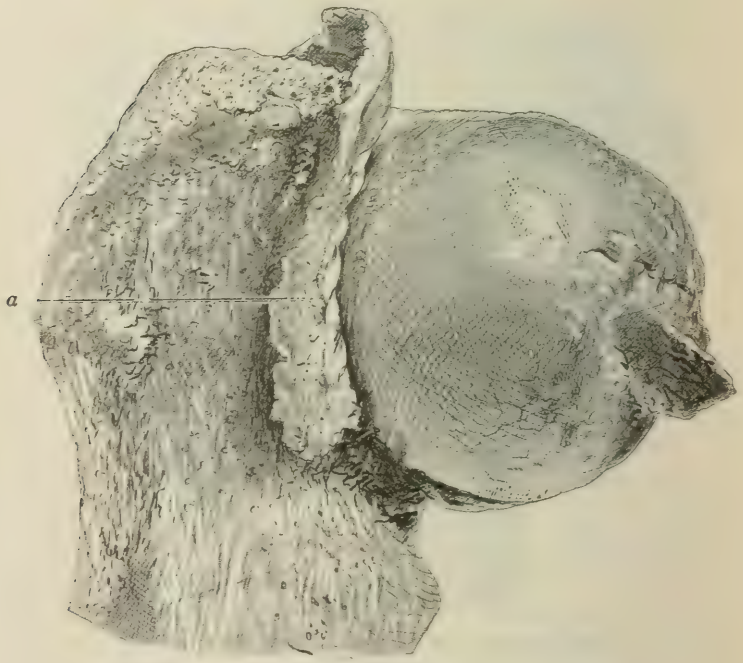
Bony Union after Intra-capsular Fracture. (Anterior view.)

a.—Line of Fracture.
b.—Capsular Ligament.

fragment. The patient eventually was able to walk quite well with the aid of a cane. Two years after the accident she died of pneumonia. The post-mortem appearances I will quote from the former report:—

"The capsule of the joint, especially the upper portion, was thickened and firm, and bridges of fibrous bands connected the line of fracture with the anterior portion of the ligament. On the anterior surface of the neck the direction of the fracture could be clearly traced from below upward, and from within outward, but not extending beyond the insertion of the capsular ligament. The line of fracture is elevated, and presents a serrated appearance. Posteriorly the head of the bone was in close proximity to the posterior inter-trochanteric ridge. A slight depression on the articular cartilage marked the point of contact with the inner surface of the capsular ligament. Impaction had evidently taken place at

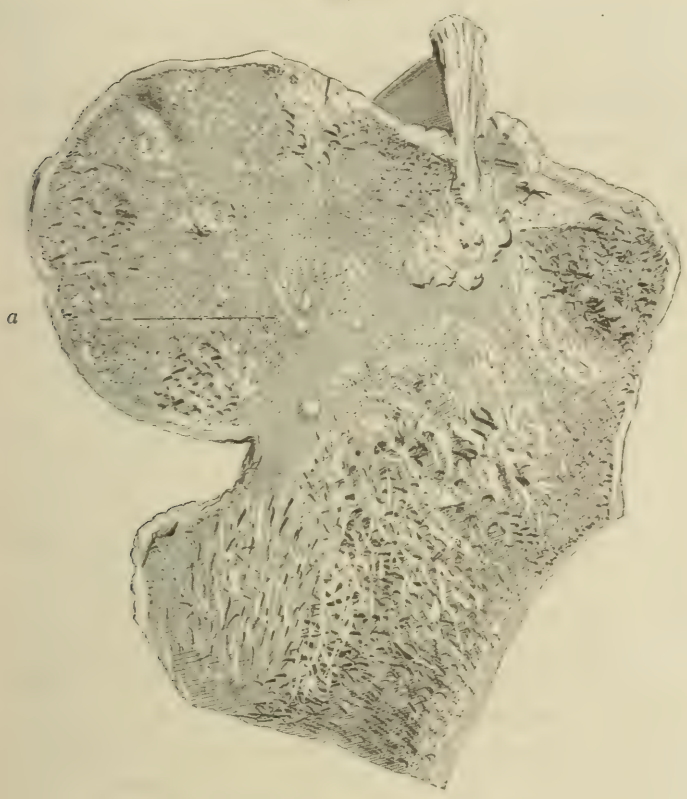
Fig. 2.



Bony Union after Intra-capsular Fracture. (Posterior view.)
a —Capsular Ligament.

the expense of the posterior compact portion of the neck. A portion of Adam's arch, which had been implanted into the lower fragment, could be distinctly seen in the spongiosa, on making a vertical section (Figs. 1, 2, 3).

Fig. 3.



Bony Union after Intra-capsular Fracture. (Vertical section.)
a—Compact Plate of Bone.

"A vertical section through the neck, head, and trochanter revealed a white line of very compact bone traversing the cancellous tissue of the neck near the shaft in an oblique direction, corresponding to the line of fracture on the anterior surface of the neck. The anterior half of the specimen has been submitted to the boiling test without affecting the union of the fragments, hence there can be no doubt as to the union by bone.

"The bone outside of the capsular ligament presents no sign of callus or any other evidences of injury or disease."

III. *Anatomy of the Neck of the Femur.*

The neck of the femur is that portion of the upper extremity of the bone located between the head and trochanters. It is a continuation of the shaft, and springs from an oblong rhomboidal surface from the inner side of the trochanteric region, and is directed obliquely upward, inward, and a little forward. Its base is limited anteriorly and posteriorly by the inter-trochanteric lines; above and behind it reaches the summit of the greater trochanter, while below and in front it extends to the upper margin of the lesser trochanter. In animals the neck of the femur is exceedingly short, and usually set almost at a right angle with the shaft. The great length and obliquity of the neck is peculiar to man. It is this which gives elasticity, freedom, and grace to the motions of the body, but at the same time its great length increases the liability to fracture. The decided inclination of the neck forward naturally turns the lower limb a little outward, and constitutes an important determining element in the production of posterior impaction and eversion of the limb in case of fracture. The length and obliquity of the neck vary at different ages and under different circumstances, in harmony with the requirements of age or conditions. In children it is very oblique, short, and slender, and the bony prominences about the hip are not well defined. As old age advances, the neck in some instances undergoes remarkable changes in structure, contour, and direction; its obliquity in many instances diminishes to such an extent that the head descends below the level of the superior border of the greater trochanter, at the same time its length is greatly diminished. During adult life, when the femur represents the most perfect degree of development and maximum power of resistance, the neck forms an obtuse angle with the shaft varying in degree from 120° to 130°.[1]

[1] Fr. Merkel, Betrachtungen über das os femur. Virchow's Archiv f. Pathol. Anat. Band 50, Heft. II.

Viewed from the front or from behind, it is seen that the neck is widest at its base or trochanteric portion, gradually becoming narrower until its most contracted part near the head is reached, when it abruptly expands into the corona of the head. The posterior surface is smooth for the play of muscles, especially the obturator externus; convex in the vertical and concave in the longitudinal direction.

The anterior surface is rough, flat from above downward, and slightly concave from without inward. On account of the direction of the neck forward the anterior surface is somewhat shorter than the posterior. According to Heppner this difference amounts to two and two-and-a-half lines.[1]

The under surface is smooth and forms with the inner side of the shaft a well-marked curve or arch nearly three inches in length, the upper surface is rough and only half the length of the lower.

The thin compact layer of bone on the anterior and upper surface is perforated by numerous foramina for the passage of nutrient vessels. Seen from above, the neck appears much narrower than from an anterior or posterior view. The proportion of the diameter of the neck from above downwards, and from before backwards, at the trochanteric portion is as two to one. From the general anatomical description, it becomes apparent that the neck of the femur is constructed strictly on purely mechanical principles, in order to resist the greatest amount of vertical pressure, but this becomes more evident if we study the inner architecture of the upper portion of the femur, and more particularly the neck. On making a vertical section through the head, neck, and upper part of the shaft of the femur, it may be seen that the thick compact layer of the shaft, as it approaches the trochanteric region, gives off from its inner surface cancelli at regular intervals, which build up the interior or spongy tissue of the bone. As the spongy tissue is formed at the expense of the compact layer, the latter gradually becomes thinner as it approaches the head, which it supplies with a delicate layer of

[1] C. L. Heppner, Beobachtungen u. Untersuchungen über eingekeilte Schenkelhalsbrueche. Med. Jahrb. Band XVIII. p. 106.

uniform thickness. The most important part of the cortical layer in relation to fractures of the neck is the lower and inner portion, which, from the support it gives to the head and neck has been called Adam's arch, or femoral brace. It is a continuation of the cortical layer of the inner portion of the shaft, preserving its thickness and strength to a level with the upper margin of the trochanter minor, where it is gradually broken up into spongy tissue, until at the corona it is lost in the thin cortical layer surrounding the head. The trochanter major is surrounded by a thin layer of compact tissue. Transverse sections made through the neck at different depths show that while the anterior and posterior layers are of the same thickness in the upper part of the neck, the posterior wall gradually loses in thickness, until near the lower surface it is reduced to the thinness of paper, while the anterior wall becomes of great thickness and strength.[1] The direction of the neck, the concavity of the posterior surface, and the thinness of the posterior compact wall as compared with the anterior, afford abundant and satisfactory explanations concerning the frequency of posterior impaction in fractures of the neck, and outward rotation of the limb in the non-impacted form.

Of great interest to the surgeon is the arrangement of the cancelli of the spongy tissue in the upper part of the femur. It is true that some of the peculiarities of the structure of the spongiosa in this locality were known to surgeons for a long time, still the credit is due to H. Meyer of having first, in 1867, called attention to the regularity of its construction, and out of this deduced distinct and positive laws governing its mechanical construction.[2]

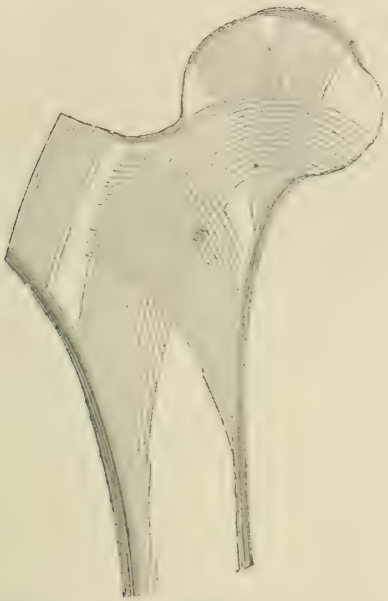
He describes three distinct systems of cancelli which traverse the neck in different directions. The first begins on a level with the lesser trochanter, and reaches in parallel curves the lower segment of the head of the femur. The second system

[1] H. J. Bigelow, M.D., The True Neck of the Femur, its Structure and Pathology. Boston Med. and Surg. Journ., Jan. 7, 1875.

[2] H. Meyer, die Architectur der Spongiosa, Archiv f. Anatomie, von Reichert und Du Bois-Reymond, 1867, p. 624.

commences on the opposite side on the same level, and traverses towards the outer side of the trochanter major, and in such a way that the points of intersection of the curves form a series of arches. The third system springs from the commencement of

Fig. 4.



Meyer's Pressure and Traction Curves. (Meyer.)

the femoral brace, and extends to the upper and inner circumference of the head of the femur, is the strongest, and transfers the weight of the body at once upon the femoral brace on the inner and lower side of the neck. According to Meyer's description and drawings, a triangle exists between the curves of the first and second systems, the apex of which corresponds to the base of the neck. This triangular space is occupied by an irregular network of cancellated tissue, or by curves from the second or third system. This space, however, does not exist in all specimens, but is usually present in the bones of aged people, where the spongiosa has undergone rarefaction.

In specimens from adults the interior of the bone is filled with

arches from the inner and outer compact layer for some distance below the point indicated by Meyer. In another paper[1] Meyer dwells at length on the mechanical function of these curves, which he calls pressure and traction curves. It seems as though the compact layer is gradually lost in the spongiosa. This is well seen in a horizontal section through the long bones, more especially of the lower extremity. The lamellæ are so arranged as to correspond to pressure and traction curves, a construction by which the least possible amount of bone tissue can resist the greatest amount of force. The spaces between these curves are filled with loose cancellated tissue, which does not add materially to the strength of the bone, but serves more the purpose of forming a framework for the medullary tissue. The spongiosa appears, therefore, as the primary typical structure of bone, and the compact tissue as an accidental modification of the spongiosa constructed by local concentration of resistance curves.

Heppner divides the cancelli of the spongiosa into two strong and two less dense portions. Among the first he includes the fibres connecting the neck of the femur with the femoral brace, and from the other side extending to the base of the femoral neck. To the second portion belong the fibres in the space between the first two portions and those of the trochanter major.

About the same time that Meyer was investigating this subject, Dr. J. H. Packard published a very interesting paper on "Fractures of the Neck of the Femur,"[2] in which he calls attention to the architecture of the interior of the neck of the femur, and accurately describes and delineates the arching fibres passing from the femoral brace to the upper segment of the head of the femur. He says: "A careful examination of vertical sections of the head and neck of the femur shows, as several writers have pointed out, that a number of the cancellous columns, beginning at the upper end of the inner wall of the shaft of the bone, diverge upward to the concavity of the thin articular lamella of compact substances of the head, so as to receive the weight of the body

[1] Die Statik u. Mechanik des menschlichen Knochengerüstes, 1873.

[2] On some Points relating to Fractures of the Neck of the Femur, by John H. Packard, M.D., Amer. Journ. of Med. Sciences, Oct. 1867, p. 379.

upon their extremities. Another series of columns are found to run outward from the same point, and from a line running upward from it, to meet other columns running up inward from the outer wall of the shaft; and these two sets of columns form a series of groined arches culminating at the upper wall of the neck of the bone, a little to the inner side of the greater trochanter.

"By this arrangement the shifting of the weight towards the outer or upper portion of the head of the bone is provided for, the pressure coming in greater degree on the outer wall of the shaft, the inner wall, however, receiving its share through the inner columns of the arches. The remainder of the cancelli run in various directions, not capriciously or at random, but so as to afford in the aggregate a very strong support to the solid wall of the bone. Sometimes it may be clearly seen that they are so placed as to run as nearly as possible in the line of muscular traction; an arrangement, the mechanical advantage of which must be at once evident."

In 1870, Julius Wolff[1] published the result of his studies of the spongiosa, which he considered as standing in the closest relation to the static and dynamic forces of the bones. In his researches he made use of the mathematical calculations of Culmann, showing that interior braces intended to aid supporting a weight upon the end of a cylinder, curved like the femur, or like a crane or derrick, should be placed, in order to act to the advantage, precisely where the trabeculæ of the spongy tissue of this bone actually exist.

"Nature," says Wolff, "has built the spongy bones as an engineer would construct a truss bridge, mathematically."

A compact plate of bone (Schenkelsporn, septum) is usually found imbedded in the cancellous tissue of the neck, and to it an important function has been assigned by Merkel and Bigelow.

Merkel[2] described a structure in the interior of the neck of the femur which he called "Schenkelsporn." It consists of a wedge-

[1] Julius Wolff, Ueber die innere Architectur der Knochen und ihre Bedeutung für die Frage von Knochenwachsthum. Archiv f. Path. Anat. u. Phys. Band 50.

[2] Der Schenkelsporn. Centralbl. f. d. Med. Wissensch. No. 27, 1873.

shaped solid projection of the cortical layer, which springs from its inner surface on a level with the trochanter minor towards the median line, and penetrates into the interior of the spongiosa to the depth of 1 cm., and is lost immediately under the head of the femur on the anterior surface of the neck. It is not found in the fœtus, is fully developed in the adult, and again disappears completely during old age.

Bigelow, after describing the necessary manipulations for exposing it, says: "The septum will then be distinctly seen, as a thin dense plate of bone continuous with the back of the neck, and reinforcing it, plunging beneath the inter-trochanteric ridge in an endeavor to reach the opposite and outer side of the shaft. At its lower extremity it curves a little forward, so as to take its origin, when on a level with, from the centre, instead of the back, of the cylindrical cavity; a disposition easily seen in a transverse section of the shaft just above the trochanter minor. Or it may be said that the posterior wall of the neck forks before reaching the inter-trochanteric line, one layer being seen upon the surface, while the other dives beneath the inter-trochanteric ridge in a vain attempt to reach the outer wall of the shaft. If these views be correct, the inter-trochanteric ridge is simply a buttress erected for the insertion of muscles upon and over the true neck, by the impaction of which it is in fact often split off and detached in a mass; the force exerted by the true neck, though slight, being nevertheless an effort to resist such impaction."

The importance of this septum, when in a perfect state, becomes evident as the same author continues: "The true neck is often, at best, but an ineffectual attempt to bridge the interval beneath the trochanters, as seen in Merkel's figures, while, in the latter half of life, it degenerates into papery plates, radiating downward from a point near the lesser trochanter. Weakened in this way, both by its own tenuity and by its own slender union to the trochanteric ridge, the true neck has great practical interest for the surgeon. Even the adult femur is generally defective in construction at this point; and here occurs the most common form of fracture, namely, the posterior impacted fracture of the base of the neck."

Bigelow looks upon the septum as a continuation of the "True Neck," while Merkel has described the same structure as a projection of a portion of the posterior and inner compact wall into the spongiosa; hence, he has called it Calcar femoral or Schenkelsporn.

The relation of the septum to the trochanter minor is such as to isolate its spongy tissue, and on that account Wolff alludes to it as "the compact tissue upon which the trochanter minor rests."

The importance of this structure in preventing and determining the location of fractures has, undoubtedly, been over-estimated. It is absent in children, and is subject to the same degenerative changes as the remaining portions of the cortical layer and the spongiosa.

In conclusion it may be stated that the spongiosa of the upper extremity of the femur is derived directly from, and at the expense of, the compact layer, that it is constructed upon a definite plan in a series of arches arranged in such a manner as to resist the greatest amount of vertical pressure according to well-established architectural laws; at the same time by expanding the bone it furnishes a more extensive surface for muscular attachments, and imparts to it a greater degree of elasticity.

The exact point of insertion of the capsular ligament into the femoral neck has been a frequent source of dispute among anatomists and surgeons to decide the location of fractures in this locality. Great discrepancies on this point may be found in our anatomical text-books.

To Dr. Geo. K. Smith much credit is due for the light he has thrown on this subject.[1] By injecting the capsule of the hip-joint with air or plaster of Paris he was enabled to make very careful dissections, and by measuring accurately the distance between the corona of the head and the insertion of the ligament, and from this point to the inter-trochanteric lines, he could locate the exact point of insertion. After preparing and

[1] The Insertion of the Capsular Ligament of the Hip-joint, and its Relation to Intra-capsular Fracture. New York, 1862.

measuring sixty-one specimens in this manner he came to the following important conclusions:—

"1st. That scarcely any two specimens of the normal capsule, taken from different subjects, are alike in their insertion into the neck of the bone; consequently no definite description of its insertion can be given.

"2d. That the normal capsules of the opposite femurs of the same subject are alike in their insertion. Having measured twenty-four pairs I have yet to see a single variation from this rule. Moreover, it is just what we must expect to find in obedience to that law of symmetrical conformation which pervades the animal economy."

Morris[1] locates the femoral insertion of the capsular ligament as follows: "At the femur the capsule is fixed to the anterior portion of the upper border of the great trochanter, and to the tubercle of the femur close to the insertion of the gluteus minimus and the origin of the vastus externus, with slips from each of which it is blended. Thence it runs along the upper and outer part of the anterior inter-trochanteric line, but it soon gets below it, and at the inner border of the femur is on the level of the lower surface of the small trochanter. It is then inclined upward and backward along an oblique line, two-thirds of an inch in front of the small trochanter, to reach the back of the neck; here it is attached above the posterior inter-trochanteric line at a distance varying from half an inch, at the lower and upper ends, to over two-thirds of an inch opposite the middle of that line. . . . After laying open the capsule it is seen that some of the deeper fibres of this ligament are reflected upward along the neck, so as to be attached to the femur much nearer its head. These reflected fibres occur at three places, one corresponding in position to the middle of the ilio-femoral ligament, another to the pectineo-femoral, and the third on the upper and back part of the neck. A thin fold of synovial membrane stretches between the reflected and the unreflected fibres of the capsule."

These strong bands of reflected fibres have been called reti-

[1] The Anatomy of the Joints of Man. Philadelphia, 1879, p. 322.

nacula by Weitbrecht. Packard, in his paper previously alluded to, has called attention to the existence of pockets, usually three in number, between the reflected fibres.

Rüdinger,[1] in speaking of the fibrous capsule of the hip-joint, says: "Its attachment to the lateral portions of the neck is in such a manner, that anteriorly it reaches the anterior inter-trochanteric line; posteriorly, on the other hand, it reaches only as far as the middle of the neck."

Practically, we can safely say, that the anterior portion of the capsular ligament, as a rule, is inserted into the anterior inter-trochanteric line, reaching its lowest level on the lower surface of the lesser trochanter, while the posterior portion is usually inserted at or near the middle of the femoral neck. A close inspection of the posterior segment of the capsular ligament will usually reveal the existence of more or less fibres running from the insertion of the ligament proper towards the inter-trochanteric ridge. For our purpose it is also proper to mention that the largest nutrient vessels to the neck pass beneath the reflected portions of the ligament.

The ligamentum teres will receive mention only as far as it serves as a carrier of bloodvessels to the head of the femur. In infants it contains a number of bloodvessels which enter the head of the femur at the point of insertion of the ligament in the fovea. That the blood supply from this source is not essential for the growth and development of the upper extremity of the thigh-bone, is apparent from the fact that it is wanting in many animals, as the Bradypus, Echidna, Cholaepus, elephant, rhinoceros, and others. According to Savory, two hip-joints were examined at the St. Bartholomew's Hospital, where no trace of this ligament could be found, but the head of the femur presented a depression, not covered with cartilage, at the usual point of insertion of this ligament. There can be no doubt that in infants the vessels of this ligament penetrate the bone and furnish material for ossification to the epiphysis of the femur. After the growth of the bone has been completed, the vessels

[1] Topographisch-chirurgische Anatomie des Menschen, Stuttgart, 1873, p. 142.

diminish in size with advancing age, and disappear entirely at their point of entrance into the bone in a certain percentage of cases. Hyrtl has asserted that the vessels from the capsule which pass along the ligamentum teres to the surface of the head do not communicate with the vessels of the medullary substance, but return in the form of loops.

In one-third of the specimens examined by Welcker,[1] no foramina nutritia could be found in the fovea capitis; hence, he concluded that the round ligament is not destined to act as a carrier for nutrient vessels. Sappey, on the other hand, attributes to the ligamentum teres the function of protecting the vessels and nerves that nourish the head of the femur.

Langer[2] has made careful injections of the vessels of the hip-joint, and has observed that not all of the vessels of the ligamentum teres return in loops on reaching the bone, as described by Hyrtl, but that some of them penetrate into the spongiosa of the head of the femur. I have examined a number of specimens with reference to this point, and have been able to detect with the naked eye nutrient foramina in the fovea capitis in more than one-half of them. When the foramina were of considerable size, I have been able to trace them for some distance into the bone. In such specimens there can be no doubt as to the presence of bloodvessels from the round ligament in the interior of the spongy tissue of the head of the femur, and in this the vessels from the round ligament enter into anastomosis with vessels from the bone, and render material assistance in maintaining the circulation in the head of the femur. In some specimens the foramina may be so small as to elude detection with the naked eye, but may nevertheless admit small vessels which may be of some importance in furnishing material for nutrition in the bone. The absence or destruction of the vessels of the ligamentum teres may not interfere with the growth and development of the bone as long as the normal supply through the neck of the femur is not impaired; but in case of fracture of the

[1] Virchow u. Hirsch's Jahresbericht, 1875, vol. i. p. 9.

[2] Rüdinger, op. cit., p. 144.

neck they are of great importance, and assume a compensating function in harmony with similar processes in other parts of the body. Rudimentary small vessels may assume vicarious action for the purpose of answering an increased demand, and thus supply a sufficient amount of blood, not only for maintaining the vitality of the upper fragment, but likewise to furnish material for repair.

IV. *Classification of Fractures of the Neck of the Femur.*

Since the teachings of Sir Astley Cooper on this subject, it has been customary to classify fractures of the cervix femoris according to the relative position the capsular ligament bears to the seat of fracture, into the intra-capsular and extra-capsular fractures, to which has been added a third variety, fractures partly within and partly without the capsular ligament. The mixed variety has given rise to a good deal of confusion, as some have included it among intra-capsular fractures, while others class it with extra-capsular fractures. Since it has been ascertained that many of the fractures of the neck of the femur are impacted, those who have placed great prognostic and therapeutic importance upon this condition, have made impaction the basis for a new classification, and speak of impacted and non-impacted fractures of the neck of the femur. Among those who favored this classification may be enumerated Cloquet, Josselin, Duplay, Bigelow, Bryant, Hueter, and Lossen.

The distinction into impacted and non-impacted fractures is important in a clinical, diagnostic, prognostic, and therapeutic sense, while the division into intra-capsular and extra-capsular fractures has a very important pathological significance. Fractures of the neck of the femur with impaction will unite by bony union irrespective of the situation of the capsular ligament, provided the impaction is maintained for a sufficient length of time. Fractures, impacted or non-impacted, outside of the capsular ligament will unite in the same manner as fractures in any other locality, if the fractured ends are kept in apposition and immobilized for the necessary length of time. Fractures at the narrow

part of the neck, and entirely within the capsule, can only unite by bone, if the penetration is such as to secure apposition for a number of weeks, or if the same degree of apposition and immobilization is affected by surgical procedures. The frequency with which impaction occurs in the femoral neck, and the important part it performs in the reparative process, entitle it to a permanent place as a basis for classification.

When we are able to diagnosticate the existence of an impacted fracture of the neck of the femur all efforts to locate the exact seat of fracture are perfectly useless, as it could have no influence in selecting therapeutic measures, and might eventuate disastrously by abolishing the most favorable conditions for a fortunate issue. If we adopt the proposition, that fractures of the femoral neck with penetration can, and often do, unite by bone, irrespective of their relative position to the capsular ligament, then the distinction between fractures within and without the capsular ligament can only find a practical application in the examination of specimens to prove or disprove the correctness of the proposition. This holds the more true as, *in vivo*, all known diagnostic means have proved unreliable in locating the exact point of fracture. The sooner the profession can be convinced that intra-capsular fractures also unite by bony union under certain favorable conditions, the better will it be to abandon the old classification, which has proved to be incorrect anatomically, and unwarranted by pathological facts. Practically, then, it is always important to ascertain the presence of impaction, and not to interfere with it when found; theoretically, and for the purpose of adopting therapeutic measures, it is desirable in non-impacted fractures to locate as nearly as possible the seat of fracture without inflicting unnecessary violence.

In the light of recent anatomical investigation and pathological research, and for the purpose of avoiding unnecessary confusion, it would be advisable to limit the term intra-capsular to all fractures that do not extend beyond the insertion of the capsular ligament, and include among the extra-capsular fractures the so-called mixed and purely extra-capsular fractures. Remembering the attachment of the anterior portion of the capsular ligament

we would naturally infer that purely extra-capsular fractures, without further injury to the shaft of the femur, if possible at all, must be exceedingly rare. The greatest number of extra-capsular fractures, as described by our text-books, have belonged to the mixed variety: intra-capsular in front, extra-capsular behind. In speaking of extra-capsular fractures, Dr. R. W. Smith says: "All extra-capsular fractures are, in the first instance, also impacted fractures, and all impacted fractures are necessarily accompanied by a fracture traversing some part of the trochanteric region. I have omitted no opportunity of investigating this point, and have now examined here and elsewhere upwards of one hundred specimens of the extra-capsular fracture, and have found in all, without a single exception, a second fracture traversing some portion of the inter-trochanteric space."

In commenting upon the paper of Dr. G. K. Smith,[1] Dr. Post suggested to substitute the terms intra-cervical and extra-cervical for intra-capsular and extra-capsular, the latter designation to indicate an impacted fracture at the base of the neck with more or less injury of the femoral shaft. As under this classification intra-cervical fractures would include intra-capsular and mixed fractures, and the term extra-cervical would imply the existence of a fracture rather beyond than in the cervix itself, these terms do not convey sufficiently accurate anatomico-pathological precision to recommend themselves for general adoption, although they are full of practical significance. Inasmuch as the principal object in writing this paper has been to prove that bony union after intra-capsular fractures can take place, the terms intra-capsular and extra-capsular have been retained, but will be applied in the sense previously suggested.

V. *Relative Number of Intra- and Extra-capsular Fractures.*

The inability to accurately locate the fracture during life, the existing confusion and uncertainty as to the meaning and application of the terms intra- and extra-capsular in the description

[1] Op. cit., p. 35.

of specimens have rendered the statistics on this point unsatisfactory and unreliable. Although the cervix femoris may be broken at any point between the head of the femur and the inter-trochanteric ridges, there are certain points where it is more liable to give way. The exact location of the fracture is determined to a great extent by the seat and degree of senile osteo-porosis, and the direction of the fracturing force. Senile osteo-porosis, as we have seen, begins in the spongiosa and reaches its maximum degree soonest at the contracted portion of the neck; hence fracture nearest the head is most likely to take place in decrepit old people. Fractures at this point are exceedingly rare in persons less than fifty years of age; only a very few well-authenticated cases being on record. Rodet, in a series of experiments on the femur and on plaster-of-Paris casts of the upper extremity of this bone, has demonstrated the important fact, that the situation and direction of a fracture of the neck of the femur may be predicted to almost a certainty by a knowledge of the direction in which the force was applied. Thus, a force acting vertically will produce an oblique intra-capsular fracture; a force acting from before backward a transverse intra-capsular fracture; one from behind forward a fracture partly within and partly without the capsule; and a force applied transversely a fracture entirely without the capsule. Clinical evidence has repeatedly verified the correctness of these observations. The traction fractures described by Linhart, Riedinger, and Hueter, from the powerful traction of the ilio-femoral ligament, when the thigh is over-extended and adducted, invariably fall outside of the limits of the capsule.

Bonnet believed that the line of fracture was almost always without the capsule, and Nélaton contended that in the great majority of cases he made the same observation; while many authors, equally competent, among them Sir Astley Cooper, Ashhurst, and Druitt, claim that intra-capsular fracture occurs more frequently in persons above fifty years of age. Of twelve specimens examined in the museum of St. Bartholomew's Hospital by Stanley, six were supposed to be intra-capsular, and six extra-capsular.

Malgaigne[1] examined one hundred and three specimens from different sources to determine the relative frequency of these fractures, and found that sixty-one belonged to the intra-capsular to forty-two of the extra-capsular variety.

M. Mercier, at Bicêtre, found in eight autopsies three intra-capsular to four extra-capsular fractures, and one below the trochanters; while Malgaigne himself, in the same hospital, found in eight other autopsies one fracture below the trochanters, five within the capsule, and only two outside of it. Stimson[2] made a post-mortem examination in six cases, and ascertained that in two of them the fracture was purely intra-capsular, and in four the fracture was at the junction of the neck and shaft.

Heppner[3] gives a description of five cases of impacted fractures of the neck of the femur, of which number three were extra-capsular and two intra-capsular. Of twenty-three specimens of fracture of the neck of the femur, in the Museums of the College of Physicians, Philadelphia, and the University of Pennsylvania, examined by Agnew,[4] ten were within and thirteen without the capsular ligament.

Mussey's collection contains twelve examples of fracture of the femur without the capsule, and ten within. The above statistics embrace one hundred and eighty-five post-mortem specimens, of which number ninety-nine were fractures within, and eighty-six without the capsular ligament; figures which would tend to prove that intra-capsular fractures are more frequent than fractures without the capsule. It must, however, be remembered that many of these specimens were collected for a special purpose, and on that account the numbers do not represent the true proportion as it actually exists. If the statistics obtained by the examination of post-mortem specimens are not reliable in ascertaining the relative frequency with which these fractures occur, the information derived from clinical observation

[1] Treatise on Fractures. Translated by J. H. Packard, M.D., 1859, p. 533.

[2] A Treatise on Fractures, 1883, p. 491.

[3] Beobachtungen u. Untersuchungen über die Schenkelhalsbrueche. Oestr. Med. Jahrb. Heft. 3 u. 4, 1870.

[4] Principles and Practice of Surgery, 1878, vol. i. p. 931.

must prove still less satisfactory in deciding this question, as the symptoms during life are not sufficiently well marked to enable the surgeon to locate with certainty the exact seat of fracture.

Billroth[1] refers to twenty-seven cases of fracture of the neck of the femur, of which number thirteen were diagnosticated as intra-capsular, and fourteen extra-capsular. In Dr. Hyde's table of three hundred and twenty-one cases of fracture of the femur, we find that the neck was involved thirty-one times, these were supposed to be located fourteen times within and seventeen times without the capsule.

Hamilton[2] has recorded eighty-four cases of fracture of the femoral neck from his own personal observation; of these, forty were believed to be without the capsule, and thirty were believed to be within, the remainder were undetermined. These statistics furnish one hundred and twenty-eight cases with fifty-seven intra-capsular, and seventy-one extra-capsular fractures, a majority in favor of the extra-capsular variety.

Combining the figures from the museum specimens and those taken from bedside observation we obtain three hundred and thirteen cases of fracture of the neck of the femur, of which number one hundred and fifty-six were supposed to be located within, and one hundred and fifty-seven without the capsular ligament.

VI. *Incomplete Fractures of the Neck of the Femur.*

The structure of the neck of the femur in the aged furnishes conditions unusually favorable for the occurrence of partial or incomplete fracture. Although this form of fracture has received but little attention on the part of surgical writers, receiving at the best only brief mention, it would appear from the cases reported during the last few years, that the accident is not as rare as has been supposed. Colles[3] was the first to call attention to this variety of fractures as it occurs in the neck of the femur, and described

[1] Chirurgische Klinik. Wien, 1879.

[2] A Practical Treatise on Fractures and Dislocations, 1880, p. 393.

[3] Dublin Hospital Reports, vol. ii.

three cases. Dr. J. B. S. Jackson, of Boston, described a case of incomplete fracture (fissure), the line of fracture extending from the junction of the upper border of the neck with the head downward to within a quarter of an inch of the inferior and internal wall of the bone. Gurlt[1] mentions three cases. In Tournel's case the infraction took place at the upper portion of the base of the neck, the line of fracture running from the digital fossa downward.

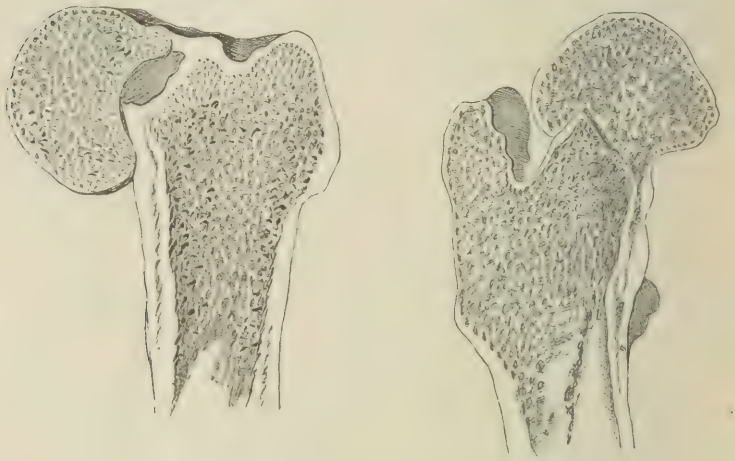
In the case reported by P. Wilkinson King, the line of fracture was near the head of the femur, a bridge of compact tissue on the anterior and upper portion of the neck, one-third the circumference of the compacta remained intact. The third described he found in the Pathological Museum in Giessen. The transverse infraction affects the entire posterior half of the femoral neck about its middle, while the anterior wall is not affected. The margins of the fractured surfaces are in immediate contact. Koenig[2] described two specimens. In the first specimen the line of fracture occurred on the upper and posterior surface of the neck, near the head, with impaction of the cervical portion into the head, while the compact tissue on the anterior and inferior surface remained entire. In the second specimen, the line of infraction took place at the lower surface of the neck, at the most constricted portion, with penetration of the apex of Adams's arch into the interior of the head, while the upper portion of the neck had yielded without being broken. (Figs. 5 and 6.) These two varieties Koenig considers as representing typical forms of this fracture, the mechanism of their production being the same as in complete fractures of the neck. In the first variety from the direction of the impaction the limb is rotated outward, while in the second form the foot remains in its natural position but the limb is shortened in proportion to the extent of the impaction. Koenig is of the opinion that many of the cases of complete recovery after supposed intra-capsular fractures, were cases of incomplete fracture with impaction. At the

[1] Knochenbrueche, vol. i. p. 31.

[2] Verhandlungen der Deutschen Gesellschaft f. Chirurgie, 1877, p. 131.

same meeting, Billroth reported two cases where he made the diagnosis of incomplete fracture during life, in both instances recovery was imperfect.

Fig. 5. Fig. 6.



Partial Fracture of Neck of Femur. (Koenig.)

Incomplete fractures of the neck of the femur, as well as of other bones, consist of a loss of continuity of a certain number of cancelli, forming the substance of bone. It may exist in every degree from a fracture almost complete to one in which the number of severed cancelli is so small as to elude detection by the naked eye. The location and direction of the line of infraction, as in complete fractures, must necessarily vary according to the direction in which the force is applied which produces the fracture. Stimson[1] says: "The line of fracture is transverse and upon the concave side, and is produced by crushing, not by overbending." Incomplete fractures are repaired by the formation of intermediate callus between the fractured surfaces which restores the continuity of the bone. The unbroken portion of the bone and periosteum serve as a perfect splint, which secures complete rest and apposition until the injury is repaired. The de-

[1] A Treatise on Fractures, 1883, p. 41.

formity attending this accident is necessarily always very slight, and as the symptoms at the same time are not pronounced, the diagnosis must always remain uncertain. The cases are most likely to be mistaken for contusion of the hip; hence, we should always examine the severer injuries about the hip with unusual care, and if any doubt exists, give the patient the benefit of such doubt, and treat the case as one of incomplete or complete fracture with impaction.

VII. *Impacted Fractures of the Neck of the Femur.*

Impaction, penetration, implantation, and incuneation, are synonymous terms, which are used to describe a fracture when one fractured end is driven into the other, an occurrence which secures perfect coaptation and fixation. In some instances impaction is mutual. Impaction may be complete or incomplete, according to the tissue-structure at the seat of the fracture, or the direction and intensity of the fracturing force. Impacted fractures are most frequently met with in the spongy portions of the long bones, and in persons suffering from osteo-porosis from any cause. These fractures have only quite recently become the object of special investigation, and are at the present time receiving the attention their importance merits.

Robert was the first to give a good description of impacted fracture of the neck of the femur and explain its mechanism. He specified the following conditions which must be present for penetration to take place. In the first place the penetrating bone must have a conical shape, and must be placed opposite a spongy section of bone, and must have been broken off close to the insertion of the same. The impacting force must be applied in the direction of the long axis of the incuneated bone. All these conditions are presented in fractures through the neck of the femur. Adams regarded the inner and lower compact tissue of the neck of the femur as the principal element of impaction; the direction of the fracture through the neck being oblique from above downward fractures the arch in such a way that the apex, sharp and pointed, is placed opposite the loosely

cancellated tissue of the shaft into which it is driven by the same force which fractured the bone.

Streubel looked upon senile osteo-porosis as the main cause of impaction. It is necessary, however, that the compacta of the fractured neck retains sufficient firmness to penetrate the bone without being comminuted. Some authors assert that impaction follows fracture in such a way that the neck of the femur gives way to indirect violence from a fall upon the foot or knee, the impaction following by the patient falling upon the trochanter. Heppner assumes that the relation existing between the neck of the femur and the trochanteric portion of the femur is the cause of impaction, and takes into special consideration the spongiosa in which he distinguishes two distinct layers, of which the one possesses a greater degree of density than the other. He believes fracture at the base of the neck with impaction is always the result of force applied to the trochanter major, which expends itself at the origin of the femoral brace, and fractures the entire base of the cervix. Aside from the diminution in the obliquity of the cervix, and the presence of senile osteo-porosis, he finds another cause for this fracture in the general atrophy of the aged, rendering the trochanter major more prominent and thus more directly exposed to external violence.

This last assertion, however, is not in accord with experience, as corpulent aged females furnish the largest number of fractures of the femoral neck. Streubel made some experiments on cadavers to determine the seat of fracture on the application of direct and indirect violence. To test the effect of violence applied in the axis of the femur, he amputated the thigh and applied the force directly to the sawed surface of the femur, and succeeded only in one instance in producing an intra-capsular fracture. By applying the force to the trochanter major he produced one extra-capsular impacted fracture, while in all other cases the trochanter major was fractured. Heppner repeated these experiments with the same results. He then reversed the direction of the force. Taking a femur, stripped of its soft parts, and resting the outer surface of the trochanter major upon a

table, he struck the head of the femur with an axe, and produced in every instance a fracture of the neck resembling an impacted fracture. He repeated the experiment thirty times, and in five of the cases the impaction was typical. From these experiments he has drawn the deduction, that the fracture is produced by contre-coup, whether the force is applied to the trochanter major or through the axis of the femur.

In regard to impaction of intra-capsular fractures he could find nothing in the literature on the subject of fractures of the femoral neck. Vollemier speaks of them at length, but only for the purpose of denying their occurrence. But, inasmuch, as he relates having seen several specimens where the end of Adams's arch was found to terminate in the interior of the spongy portion of the head of the femur, he contradicts himself, as the description corresponds with impaction of the lower wall of the femoral arch into the head. The question at issue is not the degree of impaction, but whether it can secure mutual fixation of the fragments. In most cases only the lower edge of the outer fragment is impacted, but the contrary may take place, as is evident from the description given by Koenig under the head of partial fractures.

For one of the best contributions to our knowledge of impacted fractures of the neck of the femur we are indebted to Riedinger.[1] He has studied this subject by way of experiments and examination of museum specimens. In speaking of intra-capsular fractures, he says that, as a rule, the lower and more particularly the posterior wall of the lower fragment is driven into the spongiosa of the head. As a necessary consequence of this form of impaction, the head of the femur is depressed and inclines backward sometimes to such an extent as to come in contact with the posterior inter-trochanteric line. The cortical portion of the lower fragment can often be traced into the interior of the head to a distance of two centimetres. At the anterior line of fracture the denticulated margins retain such a firm grasp as to add materially to the firmness of the impaction. At the base of the

[1] Studien über Grund und Einkeilung der Schenkelhalsbrueche, Würzburg, 1874.

neck of the femur the conditions for impaction are most favorable. If sufficient force is applied over the trochanter major, the neck fractures in such a way that the femoral brace is detached near its origin, and constitutes a sharp projection, which, when slightly dislocated, is placed *vis-à-vis* to the spongy tissue of the outer fragment, and is implanted into the same by the fracturing force. The upper portion of the inner fragment, although not possessed of an analogous dense structure as the femoral brace, follows in the penetrating process the more readily as the whole inner fragment is wedge-shaped. The spongiosa between the cortical layers forms a somewhat sharp projection. Impaction of the base of the neck is carried to its fullest extent in case the fracturing force is sufficient to fracture also the trochanteric portion of the femur. In such instances the apex of the inner fragment splits the shaft of the femur sometimes into a number of fragments, and presents itself on the outer surface of the bone beneath the soft parts.

Mr. Bryant[1] has published a table of fourteen cases of impacted fracture of the neck of the femur, and from an analytical study of these cases he draws the following conclusions:—

"1. That in all the cases the injury to the hip-joint was communicated through the great trochanter.

"2. That, as a result of the injury, there was more or less loss of power in the limb; in some cases it was complete, in as many the patient could rotate the limb slightly on the couch; and in two cases partial flexion of the thigh could be performed.

"3. That in all the cases immediate shortening of the injured limb was the direct result of the accident; and that this shortening was about an inch or less, and it was irremediable by extension.

"4. That the foot of the injured extremity was either straight or slightly everted, although in several cases this eversion was less marked on the injured than on the sound side.

"5. That the great trochanter was placed nearer the median line of the body, and also nearer the anterior superior spinous process of the crest of the ilium than on the sound side.

[1] Medical Times and Gazette, April 17 and May 1, 1869.

"6. That the head of the femur could be made to rotate smoothly in the acetabulum, and the great trochanter moved with it.

"7. That crepitus was either absent or indistinct in all cases.

"8. That all the cases, with one exception, occurred in patients past middle age."

Bardeleben[1] maintains that, in intra-capsular fractures, longitudinal displacement is opposed by the untorn portions of the capsular ligament.

In this fracture the ends of the fragments are often interlocked in such a manner as to prevent dislocation, and may even enable the patient to walk on the limb for a few hours, or for several days.

The more important elements in retaining the fragments are, however, the presence of impaction, and the untorn portions of the reflected capsule, the retinacula of Weitbrecht.

S. D. Gross[2] believes that impaction is rare, and, when present, it is almost exclusively extra-capsular. The distance of penetration varies from a few lines to one-half or three-quarters of an inch.

Hueter[3] places great importance in recognizing the presence of impaction. He regards the schenkelsporn as the most important agent in the process of impaction. Anatomically he distinguishes two varieties: either the upper end of the lower fragment is displaced inward, so that the termination of the schenkelsporn penetrates the soft tissues below the upper fragment, or the lower fragment is displaced outward in such a manner that the schenkelsporn is driven into the spongiosa of the neck. Impacted fractures are not so frequent as non-impacted fractures, but they are sufficiently common to render them of the greatest importance in diagnosis, prognosis, and treatment of fractures of the neck of the femur.

H. H. Smith[4] believes that in the majority of cases the neck

[1] Lehrbuch der Chirurgie, 1871. Band II. S. 473.

[2] System of Surgery, 1864, vol. i. p. 965.

[3] Grundriss der Chirurgie, 1882. B. II. S. 883.

[4] The Principles and Practice of Surgery, 1863.

of the femur is fractured by indirect violence, impaction following subsequently by a fall upon the trochanter major.

R. W. Smith says: "That all extra-capsular fractures are, in the first instance, also impacted fractures."

Robert[1] was of the opinion that fractures of the neck of the femur were nearly always impacted, and as such should be disturbed as little as possible to obtain the best results, as the impaction furnishes the best possible conditions for bony union to take place.

MacNamara[2] affirms that fractures of the neck of the femur are usually impacted, the fragments being jammed into one

Fig. 7.

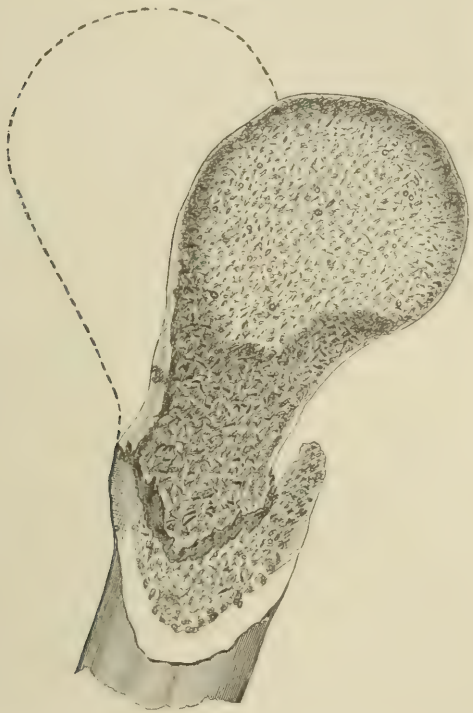
Posterior Impaction of Femoral Neck. (Bigelow.)

another; the smashed cancellated tissue must be removed, rendering the process of repair tedious.

[1] Mémoire sur les Fract. du col de Fémer, 1845.
[2] Diseases of Bones and Joints, 1881.

Bigelow,[1] who has devoted a great deal of time and attention to the subject of injuries about the hip-joint, from the views he entertains as to the architecture of the femoral neck, is convinced that fracture takes place most frequently at the base of the neck, and is usually accompanied by impaction of its posterior wall. (Figs. 7 and 8.) These cases present outward rotation of the limb, and slight shortening, and may be followed by complete repair without lameness.

Fig. 8.



Posterior Impaction of Femoral Neck. (Bigelow.) Transverse Section.

Impaction at the constricted portion of the neck is not frequent. Impaction of the entire base of the neck with inward

[1] The True Neck of the Femur, Boston Med. and Surg. Journ., Jan. 7, 1875.

rotation of the limb is very rare, and is hardly possible without fracture of the trochanters.

The same author, at a meeting of the Boston Society for Medical Improvement, held November 23, 1874, exhibited a specimen of a fracture within the capsular ligament with imperfect impaction, which, during life, had simulated impaction at the base of the neck, and induced him to express a favorable prognosis. "The autopsy showed that the fracture was not through the base of the neck, but through the neck itself, close to the head, and that the fragments were 'rabbeted' together. There was motion enough to have worn away the thin walls of the neck, and to show that any bony union, had the patient lived, was not to be hoped for. In this respect it differed from Dr. Gay's case of impacted fracture into the head, where the patient, on the day of his death from pneumonia, a week or two after the accident, lifted up his leg and said that as far as that went, he was getting well. Had that man lived, he would undoubtedly have had bony union and a serviceable leg. The rabbeting of the fragments was shown here very well in the present specimen. It was due to a conical mass of comparatively dense bony tissue projecting from the head fragment, which was driven into the loose cancellated structure of the portion of the neck in the shaft fragment. This dovetailing, although sufficient while the fragments were surrounded by the capsule and soft parts to prevent crepitus, and to cause the neck and head to rotate in the socket as a whole, did not prevent such attrition of the fragments as would hinder bony union."

Koenig[1] locates fractures of the neck of the femur either near the head or the trochanteric portion, localities which correspond to intra- and extra-capsular fractures. From anatomical reasons, after a fall upon the trochanter major, the anterior wall of the neck (the convex side) fractures first, and the fractured end of the neck is directed forward. In most, if not in all cases, the wedge-shaped end of the inner fragment is implanted into the trochanteric portion, producing impaction.

[1] Lehrbuch der Speciellen Chirurgie, 1879. B. II. S. 837.

Adams's arch, the densest and strongest portion of the neck, penetrates the deepest. The greater the inclination of the inner fragment forward the more extensive the impaction. As a necessary result of this impaction, the head of the femur descends and approaches the posterior inter-trochanteric line, the dislocation of the head in these directions satisfactorily accounts for the shortening and outward rotation of the limb.

Accurate statistics as to the frequency with which impacted fractures occur as compared with non-impacted fractures, are still wanting. The individual experiences of surgeons are so widely at variance on this point, that a final decision can only be rendered after the accumulation of more positive knowledge from accurate bedside observation and post-mortem examinations.

From a study of the literature on this subject it is apparent, however, that the more recent authors advance the opinion that it is of frequent occurrence. It is also evident that impaction is not limited to any particular part of the femoral neck, but that it can follow any fracture, although the most favorable conditions for its occurrence are found at either extremity of the femoral neck. The direction and extent of impaction depend on the density of the tissues which are penetrated, and on the direction and intensity of the fracturing force. Impacted fractures within the capsule may occur from the application of indirect violence, as the capsular ligament will offer the necessary resistance; on the other hand, impacted fractures without the capsular ligament can only take place from direct violence. It is also possible in cases of this kind, as suggested by several authors, that a simple fracture is produced in the first place by force applied through the axis of the femur, and impaction takes place subsequently by a fall upon the trochanter major. Impaction from indirect violence would necessarily take place at the lower portion of the constricted portion of the neck by the apex of the femoral brace penetrating the soft spongiosa of the head, while if produced by a fall upon the trochanter major, the compacta of the posterior surface is also implanted into the head. Impaction outside of the capsule, from the normal position of the neck

and the direction of the fracturing force, always takes place at the expense of the posterior portion of the neck, except in cases where the fracturing force is so severe as to drive the entire neck wedge-like into the upper portion of the femoral shaft, splitting the latter into two or more fragments. Impaction implies the destruction or crushing of more or less of bone tissue; and, in case the fragments are unlocked, a vacuum is formed, which must be filled by the interposition of fluids or the adjacent soft tissues. It is well known that intra-capsular fractures are often produced by very slight injuries, and it is equally certain that these are the cases which furnish the most unfavorable prospects for a good result, and the question might naturally arise, had the violence been sufficient to produce deep penetration, would it not have enhanced the prospects for a more favorable issue? In fractures of the neck of the femur the prospects for a favorable result are better if the exciting cause acts with sufficient intensity to produce impaction, as this condition is the most favorable for repair by bony union.

VIII. *Predisposing Causes.*

Fractures of the neck of the femur is one of the rarest accidents during childhood and adult life, while after the fiftieth year they constitute a high percentage of all fractures. Between the twenty-first and thirtieth year they constitute $\frac{1}{91}$ of all fractures; between thirty and forty $\frac{1}{74}$;[1] between fifty and sixty nearly $\frac{1}{10}$, and over seventy $\frac{1}{3}$. Their frequency increases steadily with the advance of old age. A number of explanations have been advanced to explain this clinical fact. Thus, Richter[2] mentions the following predisposing causes: 1. Spongy texture of neck, and diminution in thickness of compact layer. 2. Diminution in the obliquity of the neck. 3. Prominence of trochanter major by which the fracturing force is transmitted directly to the neck.

Walther[3] assigns to syphilis an important part. Sex has also

[1] Gurlt, Handbuch der Lehre von den Knochenbrueche, 1862, vol. i. p. 30.

[2] Lehrbuch von den Bruechen, etc., 1833.

[3] System der Chirurgie, 1852. B. VI. S. 348.

been mentioned as a predisposing cause; aged females furnish a greater number of fractures, and it was claimed that this could be explained by the more horizontal position of the neck in the former than in the latter. As the strength of the femoral neck is derived from the peculiar architectural arrangement of the spongiosa, the simple diminution of its angle would not render it more liable to fracture, as Julius Wolff has shown that, even in fractures that have healed with considerable deformity, the structure of the spongiosa is perfectly restored in accordance with the original plan. If the neck is placed at a right angle to the shaft, it would give way more easily at the constricted portion on the application of indirect violence, while from a mechanical standpoint it ought to resist force more advantageously in case it is applied in the direction of the axis of the neck. The predisposing cause is intrinsic—inherent in the bone itself—a degeneration or diminution of bone-tissue. All influences which affect nutrition, and that of bone in particular, as insufficient or improper food, wasting diseases, prolonged confinement in bed, etc., hasten the degeneration of bone, consequent upon senile marasmus. Senile osteo-porosis then is the only known predisposing cause; an assertion which is abundantly confirmed by clinical experience.

IX. *Exciting Causes.*

Fractures of the neck of the femur are produced by—

1. Force applied in a vertical direction through the axis of the femur.

2. Force applied in a horizontal direction over the trochanter major in the axis of the femoral neck.

3. Traction force transmitted through the capsular ligament when the limb is forcibly hyper-extended and adducted and rotated outwards.

A fall upon the foot or knee, as a rule, will fracture the neck at its narrowest portion; and, if the fracture is complete, no impaction will take place, unless it follows as a secondary occurrence from transmission of force through the trochanter major.

Most authorities who believe that intra-capsular fractures are the most frequent, assert that indirect violence is the most frequent exciting cause.

Experiments and clinical observation have shown that the majority of fractures of the neck are produced by force applied in the direction of the axis of the neck by falls upon the trochanter major. It is also an established fact that in most instances of this kind the neck gives way at its trochanteric portion, and that the posterior wall is crushed or fractured first. Impaction takes place more frequently from direct force with deeper penetration of the posterior than the anterior wall of the neck.

Of thirty cases of fracture of the neck examined by Desault[1] for the purpose of learning the exciting cause, twenty-four were produced by a fall upon the trochanter major. All the cases reported by Sabatier appear to have been produced in a similar manner. Sabatier ascribed to the prominence of the greater trochanter an important part in the production of fracture, and believed that fracture of the femoral neck does not occur in children on account of the imperfect development of the upper extremity of the femur.

Although direct force through the axis of the neck generally expends itself near the femoral shaft, causing a fracture of the expanded portion of the neck with posterior impaction, there are a number of cases recorded where the fracture occurred within the capsule. Intra-capsular fractures produced in this manner are often impacted.

The last manner in which a fracture may be produced of the femoral neck is by forcible hyper-extension and rotation outward of the limb, movements by which the ilio-femoral ligament is put upon its utmost stretch; and, when this bone has become so fragile that it is unable to resist the traction of this powerful ligament, a fracture, the so-called traction fracture, takes place at the junction of the neck with the femoral shaft. This fracture

[1] A Treatise on Fractures, etc., 1817.

is always extra-capsular, and was first described by Linhart, and subsequently experimentally studied by Rüdinger.[1] Rüdinger believes the fracture takes place before the patient falls upon the ground; comminution of the trochanter major and impaction may subsequently result from direct violence.

X. *Senile Osteo-porosis.*

Senile osteo-porosis is an affection of the bones in the aged, characterized by diminished elasticity and increased fragility as compared with healthy bones. It is an excentric atrophy of bone, due to impairment of the physiological functions which preside over digestion and assimilation. During the incipient stages the bone retains its volume, but loses in density and weight, owing to an actual loss of bone-tissue, which necessarily results in increased porosity. This affection appears first in the spongiosa of the head and neck of the femur, and it is here where its most advanced stages are observed. In the upper end of the femur this senile degeneration weakens the support to vertical pressure, allowing the head gradually to descend in some instances below the summit of the great trochanter, at the same time diminishing the oblique angle of the neck to almost a right angle with the shaft. Senile, in connection with this subject, however, is a relative term, as this condition of the bones may be found in comparatively young people, provided atrophic changes peculiar to old age, the senile marasmus, from any cause manifests itself; and, on the other hand, even very old people, when otherwise in good health, may remain exempt from this form of senile degeneration. I have in my possession a femur, taken from a gypsy 104 years of age at the time of his death, which illustrates this assertion. The neck of the bone retains the normal obliquity, the compact layer is firm and thick, while the spongiosa, although somewhat more porous, presents the different systems of arches in a degree of perfection unsurpassed at any age. Senile atrophy of bone, and the diminution in the

[1] Studien über Grund u. Einkeilung d. Schenkelhalsbrueche, 1874, p. 63.

obliquity of the neck of the femur resulting from it, have justly received so much attention on the part of pathologists and surgeons, to explain the frequency of fracture, that I may be pardoned for entering more fully into a consideration of this subject.

All authorities agree in assigning to the structural degeneration of the neck of the femur an important, if not the only predisposing cause in the production of fractures in this locality in the aged. The increased brittleness of bones during advanced age has been explained in different ways. Bichat attributed it to an excess of the inorganic constituents of the bone with a corresponding decrease of the organic matter. This view found almost universal acceptance among surgeons, and is still taught in many of our text-books on surgery.

Henle was among the first to assert that the fragility is not caused by a disproportion of the earthy and organic constituents of bone, but that it is the result of an actual loss of bone tissue, a genuine atrophy of bone. Ribbert[1] has made special inquiries into the pathology of senile atrophy of bone, and from the prevalence of this affection along the lower regions of the Rhine, he has been enabled to study this process under the most favorable circumstances. On examining sections of osteo-porotic bone stained with carmine he invariably found an osteoid red zone upon the surface of the lamella varying in size, extent, and number in proportion to the severity of the process. These zones presented very delicate parallel lines, and bone-cells with very fine or spindle-shaped projections. The margins of the osteoid layers towards the unchanged portion of the lamella are not very irregular, and usually devoid of lacunæ. The medulla, as a rule, is pulpy, hyperæmic, and often infiltrated with blood, the medullary spaces dilated, and the lamellæ correspondingly reduced in thickness. In one case he found the latter completely destroyed and converted into osteoid tissue, forming cyst-like cavities filled with pulpy medullary tissue. Ribbert regards the process as being due to an impairment of nutrition, which of

[1] Ueber senile Osteo-malacia u. Knochenrésorption im Allgemeinen. Archiv. f. Path. Anat., LXXX. S. 436.

necessity is attended by chemical changes of the basis substance of bone, which is followed again by a separation of the earthy phosphates from the basis substance. The earthy salts are rendered soluble by acids (perhaps carbonic acid) contained in the fluids of the body. As an evidence that the earthy phosphates are separated from the organic tissues, the author mentions that he has seen a granular opacity at the junction of the osteoid spaces with the normal bone, which cleared up on the application of an acid.

Insufficient or improper food has an important influence in impairing the nutrition of bone, as has been abundantly shown by reliable observations. Roloff[1] made experiments on animals for the purpose of studying the pathology of fragility of bones. He was able to produce this condition artificially by depriving the animals of all food containing phosphate of lime. He ascribes the condition primarily to a diminution of the earthy constituents of bone, and during the more advanced stages of decalcification a metamorphosis of osteoid into myeloid tissue takes place. The walls of the canaliculi appear more transparent and wider, and present the appearance of light stripes with dark edges. The walls of the bone-cells or spaces become translucent. During the further progress of the metamorphosis the appearances are such as would indicate that the bony tissue over greater or less areas had been entirely destroyed, and its space filled up with new myeloid cells from the adjacent myeloid spaces. A close examination, however, showed that the myeloid cells originated from the bone-cells.

Maresch observed an epidemic of fragility of bones among cattle and pigs in Bohemia, which lasted for three years, and was attributable to a prolonged drouth, and consequent upon it poor quality of food. He invariably found that the disease showed a predilection for the extremities of the long bones, which were softened to such an extent that they could be readily cut with the knife. The interior of the spongiosa was filled with a white pultaceous mass, while the compacta was reduced to a thin shell.

[1] Ueber Osteo-malacia u. Rachitis. Virchow's Archiv, B. XXXVII. Hft. 4, S. 434.

M. Mercier observed that in all specimens of senile atrophy the alveolar spaces were enlarged, rendering the tissue more porous.

Kölliker[1] believes that giant-cells are the agents which produce absorption of bone in normal and pathological conditions. These cells, when found in bone, he calls osteo-klasts. They originate from osteo-blasts, and are found on the surface and in the interior of the lacunæ.

Humphry,[2] in mentioning the causes of fracture of the femoral cervix, alludes to the subject as follows: "In the aged the arrangement of the cancelli, which in the young and middle aged are so admirably adapted to support superimposed weight, becomes imperfect from senile degeneration, a process which begins earliest in the upper end of the femur."

Herman Meyer[3] advanced the idea that senile osteo-porosis appears under the form of a non-suppurating periostitis, the product of which as yet remains unknown.

Gurlt's[4] description of senile osteo-porosis may be condensed as follows: The medullary canal is enlarged, the cortical layer very thin or entirely absent, and the spongiosa exceedingly porous, the meshes being filled with myeloid tissue of a dark color.

Kassowitz[5] affirms that absorption of bone takes place as a consequence of increased vascularization, the loss of bone-tissue being the direct result of absorption.

G. Pommer[6] has studied the process of resorption in diseased bones with a view to determine the formation of Howship's depressions and the function of the osteo-klasts. His observations, based upon thirty cases of bone atrophy, due either to old age or pressure from any cause, has induced him to accept Kölliker's views as mentioned above.

[1] Die Verbreitung und Bedentung d. vielkernigen Zellen d. Knochen u. Zaehne. Virchow u. Hirsch Jahresb., 1872, vol. i. p. 21.

[2] Treatise on the Human Skeleton, 1871, p. 471. [3] Op. cit.

[4] Handbuch von der Lehre der Knochenbruechen, 1861, vol. i.

[5] Virchow u. Hirsch's Jahresbericht, 1881, vol. i. p. 262.

[6] Ueber die lacunaere Resorption im erkrankten Knochen, Wiener Sitzungsbericht. B. 83, Abth. III. S. 17.

Morisane has made histological examinations to ascertain the minute processes which take place in the destruction and absorption of bone in cases of rarifying osteitis. He believes that the normal exudation which takes place from the minute vessels in bone in a state of health, is destined to perform the essential part in the production of bone, but when inflammation is present, chemical changes take place in the exuded material, which annihilate the bone producing properties of the cells. He distinguishes an acute and chronic form of the disease. In the chronic form the destruction of the compact tissue is the result of a progressive diminution of its earthy phosphates, during which the basis substance is reduced to fibrillæ, which unite with the connective tissue of the marrow spaces, and undoubtedly materially assist in the formation of the connective tissue framework of these spaces. In the acute form, where nutrition is seriously impaired, the stroma undergoes rapid granular degeneration, while the earthy salts are absorbed, the bone-cells are destroyed instead of being converted into connective tissue, as is the case in the chronic form. In the chronic as well as the acute variety of rarefying osteitis, the death and absorption of bone-tissue is dependent in an intimate manner on the numerical increase and degree of dilatation of the small vessels; and, to a certain extent, it also results from a chemical change which the products of exudation have undergone; to this latter must be assigned an important rôle as a dissolving agent.

I have called attention to inflammatory osteo-porosis of bone under the head of senile osteo-porosis, in order to explain the rapid absorption which takes place after fracture of the neck of the femur. In all of these cases, and more particularly in the intra-capsular variety, more or less of the neck is removed by absorption, in some instances almost the entire neck disappears. This process of absorption after fracture expends itself more on the lower than the upper fragment, and more upon the posterior than the anterior portion of the neck. Interstitial absorption of the neck of the femur in young adults, as described by Gulliver,[1]

[1] Interstitial Absorption of Neck of Femur without Fracture, Medico-Chirurgical Review, vol. xvii. p. 543.

can only be explained on the hypothesis that inflammatory softening and absorption were induced by traumatism. Only in two cases of the four reported by Gulliver was the diagnosis verified by the autopsy. Both were young men who had suffered contusion of the hip on the affected side, in both the neck was shortened, and almost at a right angle with the shaft; in one of them the cancellated tissue of the neck was more compact, and some adventitious bony material on its surface near the shaft; while in the other case, "the cancelli of the neck were filled with caseous matter, in some nearly colorless, in others tinged with dark grumous blood."

Similar specimens have been brought forward as cases of bony union after intra-capsular fracture; there is, however, in all of these cases a symmetrical atrophy of the anterior and posterior portion of the neck of the femur, while in cases of intra-capsular fracture with bony union, there is almost without exception a greater loss of substance of the posterior than of the anterior portion of the neck.

Interstitial absorption of the neck in young adults is always the result of rarefying osteitis, which eventuates in loss of bone-tissue, but after the disease has subsided regeneration takes place, the spongiosa becomes more dense by deposit of new bone, while the same process takes place beneath the periosteum, giving rise to adventitious bone, which might easily be mistaken for callus, as in Case III. described by Gulliver; in other instances the process of destruction is not followed by repair, the products of inflammation remain in the cancelli, as in Case IV., mentioned by the same author:

There has been considerable difference of opinion in regard to the particular structure which is the primary or most important seat of senile osteo-porosis. Authors have located it in the structures which, in accordance with the views they entertained, were supposed to furnish the principal support to vertical pressure; thus Meyer found it in the spongiosa, Merkel in the calcar femoral, Bigelow in the compact tissue of the neck. Senile degeneration, in preference, affects the spongy bones, and the spongy portions of the long bones; hence, there can be no doubt

that it first attacks the spongiosa in the interior of the head and neck of the femur, from where it gradually invades the different parts of the compact tissue. With a view to prove this assertion I have examined a number of sections of the upper extremity of the femur from persons advanced in years, and have repeatedly observed the spongiosa atrophic and porous, while the femoral brace and the calcar femoral retained their normal strength. During the advanced stage of the affection the compacta may be reduced to the thinness of paper, or disappear entirely, the periosteum coming in contact with the spongiosa; in such cases, fracture takes place on the slightest application of force, and the bone can be readily cut with a knife. The myeloid spaces are the starting points of the process of degeneration; they are enlarged and become distended by the accumulation of myeloid cells and granulation tissue; the cancelli are broken down, and thus spaces of considerable size are created at the expense of the regular system of arches which make up the structure of the spongiosa. Giant cells and granulation tissue possess the property to disintegrate and absorb bone, they are the destructive agents, while the broken-down tissue is removed by the bloodvessels. When sections of a bone, the seat of senile atrophy, are examined during the earlier stages of the process, they present a vascular and red appearance, on account of increased vascularization and an abundant deposit of red marrow. When degeneration has progressed to the highest degree, the compacta has disappeared almost completely, the spongiosa is exceedingly porous, the alveoli large and filled with fat, the bone presents a yellow appearance, imparted by the free fat derived from the fatty degeneration of the histological elements.

As long as the bone is supplied with an abundance of myeloid tissue and bloodvessels, its bone-producing capacity is not impaired; on the other hand, it is said to be increased, irrespective of the degree of softening and fragility; but if fatty degeneration has progressed to the extent just mentioned, and the vascular supply is greatly diminished, as is the case in the most aggravated forms of senile osteo-porosis, no attempt at production of bone can reasonably be expected in the event of fracture. We

are then justified from a consideration of the foregoing remarks in adopting the following conclusions:—

1. Senile osteo-porosis is the only predisposing cause of fractures of the neck of the femur in the aged.

2. Senile osteo-porosis, except in the most advanced stages of fatty infiltration, does not impair, but hastens the production of myeloid, or permanent callus.

XI. *Symptoms of Fracture of the Neck of the Femur.*

As the very best authorities are forced to admit that during life it is impossible to locate accurately the precise seat of fracture, there exists no longer the necessity of considering the symptoms separately under the head of intra- and extra-capsular fracture. In practice, the greatest care should be exercised to ascertain the presence of impaction; but even impacted fractures present the most important symptoms in common with non-impacted fractures; and they may be conveniently, and I think profitably, grouped together to prevent unnecessary repetition.

The symptoms presented by a fracture through the neck of the femur, as in any other fracture, are: 1. Subjective; 2. Objective.

The subjective symptoms are: 1. Pain; 2. Loss or impairment of function.

1. *Pain.*—The pain is due either to the immediate effects of the traumatism, laceration of the contiguous soft tissues, irritation produced by the movements of the fractured ends, or the inflammation of the bone or surrounding tissues succeeding the injury. The pain is variable, almost absent, and of short duration in some cases, excruciating and continuous for months and sometimes years in others. If the fracture is located in the narrow portion of the neck, the pain is usually referred to the groin about the insertion of the ilio-psoas muscle; if at or near its base, it is more diffuse, and referred to the seat of injury.

There has been considerable discrepancy of opinion as to the severity of the pain in fractures within, as compared with fractures without the capsule. Sir Astley Cooper maintained that

it is less severe in the former variety, while Malgaigne claimed that the reverse was true. As fractures of the narrow portion of the neck are the result of less violence than when they occur near the shaft, it is undoubtedly true that the pain attending them immediately after the traumatism is less than in the latter class of injuries, while the reverse may be true during the subsequent history of the case. In impacted fractures, where the favorable conditions for bony union are not disturbed, and the process of repair is initiated at once, and progresses uninterruptedly, the pain, as a symptom, is referable only to the traumatism; and as such, as a rule, is more severe in fractures where the greatest amount of tissue has been lacerated, that is, in extra-capsular fractures.

In cases of non-impacted fractures within the capsule, with motion of the fragments upon each other, a certain amount of inflammation springs up, which is always attended by its most prominent symptom—pain. When pain, the result of inflammation, is present, it assumes the characteristic features as witnessed in coxitis independent of fracture. It is then no longer a symptom of fracture, but indicates the accession of traumatic coxitis. The presence of no inconsiderable amount of inflammation has repeatedly been verified at post-mortem examinations, in the form of thickening of the capsule, adhesions, and destruction of the synovial membrane and cartilage. Any attempt at motion or pressure against the trochanter major aggravates the pain. In some old inveterate cases the pain assumes a neuralgic type, which would indicate that some of the nerves about the hip-joint were encroached upon by the displaced fragments, or exuberant callus, or the products of inflammation.

2. *Loss or Impairment of Function.*—This symptom is present in all fractures of the femoral neck. As a general rule, it may be stated, it is prominent as a symptom in proportion to the degree of separation of the fragments. In impacted fractures the patients are often not only able to move the limb, but even walk for hours and sometimes days. The impairment of voluntary movements does not depend alone on the direct loss of support, but is also influenced by the pain incident to such

movements; hence, this symptom will present itself in the highest degree in nervous, excitable patients. Laceration of the soft parts, the periosteum, and capsule, in the absence of impaction, will also counteract voluntary motion, not only by allowing a greater degree of disjunction of the fragments, but likewise by increasing the pain on any attempt at motion.

In the great majority of cases the patient, as he lies in bed, is unable to raise or move the limb in any direction, it remains perfectly helpless in the position it was left after the accident, or in which it has been left by the displacing elements. In some cases where interlocking of the fragments exist, or where a slight amount of impaction has taken place, the patient has control over a certain amount of voluntary movements for a number of days, or until disjunction of the fragments takes place as a result of injudicious examination or inflammatory osteo-porosis, when the limb is placed in the same conditions as if no impaction had taken place.

The objective symptoms are: 1. Swelling and deformity at the hip. 2. Suggillation about the hip. 3. Eversion of limb. 4. Shortening. 5. Change of position of trochanter major. 6. Either increased or diminished mobility of the hip-joint. 7. Loss of tension of fascia lata between the trochanter major and the crest of the ilium.

1. *Swelling and Deformity.*—In all cases there is an appreciable fulness in the fold of the groin corresponding to the seat of fracture. This swelling is caused by the hinge-like projection of the anterior portion of the neck, effusion of blood or inflammatory products, and, lastly, by the over-riding or impaction of the fragments. When impaction takes place at the base of the neck, the trochanteric portion of the femur is enlarged from implantation of the upper fragment. The swelling is larger when the fracture is located without the capsule from the more extensive bone injury and the more copious effusion of blood.

2. Suggillation appears earlier and more constantly the nearer the fracture is seated to the femoral shaft. As this symptom is the result of the presence of blood at the point of fracture, it is more extensive if the hemorrhage has been considerable and

outside of the capsule. If the hemorrhage has been within the capsule, and the capsule is ruptured at some point, the discoloration will usually show itself along the inner side of the thigh. The same force which produced the fracture may also contuse the soft parts sufficiently to give rise to superficial discoloration independent of the fracture.

3. *Eversion.*—The lower limb in a natural condition is slightly everted on account of the forward obliquity of the femoral neck. This normal eversion is increased during sleep when the muscles are at rest, or when they have been completely relaxed by an anæsthetic, or when their action has been permanently suspended by paralysis. In the normal condition, then, the weight of the limb effects outward rotation until arrested by muscular action, or the resistance offered by the ligament of the hip-joint. As the posterior wall of the neck is usually the seat of more extensive comminution or impaction than the anterior, and as the fracturing force in the majority of cases is applied in the antero-lateral direction, it is only reasonable to expect that outward rotation of the limb is the rule. And this expectation has been verified by clinical observation. Until recently it has been generally supposed that eversion is the result of muscular contraction, and in support of this view it has been suggested that in non-impacted fractures it increases after the muscles have recovered their contractility.

Edmund Owens, on the ground of anatomical demonstrations and carefully made experiments, as well as accurate clinical observation, holds that eversion of the limb takes place independent of muscular contraction, that it is invariably the result of the impacting force or the weight of the limb as the case may be. In intra-capsular fractures it is especially true that eversion is more marked a few days after the injury, but this fact can be interpreted more satisfactorily from a different standpoint. In such cases the fragments are often kept in apposition by an interlocking of the broken surfaces or unlacerated portions of the fibrous investment of the neck; either of these supports may give way to the constant traction from the weight of the limb, or the same result may take place from reflex muscular contractions,

or careless handling of the limb. The great mass of muscles, the external rotators of the hip, after the fracture, are relaxed from the approximation of their points of origin and insertion, and it is difficult to conceive in what way they could effect outward rotation. Dupuytren believed that eversion may also be due to the action of the adductor muscles, and in some instances to the obliquity of the fracture itself. It is also necessary to mention that eversion is not a constant symptom. Cases have been described by reliable observers where the limb remained normal so far as the position of the foot was concerned, and in some even the reverse, inversion occurred. Cases of fracture with inversion have been described by Ambroise Paré, J. L. Petit, Guthrie, Stanley, Dupuytren, Desault, Cruveilhier, Hamilton, R. W. Smith, and others. Desault thought that it occurred in about one case out of every four. Stanley observed one case where the autopsy showed that the fracture was purely intra-capsular, and no satisfactory explanation could be found for the inversion. Wm. Pirrie mentions a case of intra-capsular fracture where the limb was not only inverted but also strongly flexed and adducted, a position he ascribed to the tension of the ilio-femoral ligament. Of the one hundred and thirty cases of intra-capsular fracture of the neck of the femur which came under Pirrie's observation, and where the accuracy of the diagnosis was verified by dissection, this was the only case with flexion, adduction, and rotation inward of the limb. Of the remaining number, in one case only existed inversion, the limb in other respects occupying the usual straight position. Malgaigne[1] reports an exceedingly interesting case. "In 1833, having found the foot inverted in a fracture of the neck of the femur, I ascertained that it was easily everted and again inverted at will, and that it remained as readily in one position as in the other; *whence I concluded that whatever inclination is given to the part upon the supporting plane it keeps by its own weight.*"[2] This observation is exceedingly valuable, and would lead us to the conclusion that whenever the support derived from the cervical portion of the femur is lost, the limb

[1] A Treatise on Fractures. Translated by J. H. Packard, M.D., p. 543.

[2] Italics my own.

will follow the natural law of gravitation, and will turn outward by its own weight, unless opposed by some special conditions at the seat of fracture, or by external influences.

4. *Shortening.*—The significance of shortening as a symptom of fracture of the neck of the femur has received additional interest, since it has been ascertained that in many persons there is naturally a difference in the length of the lower extremities in the same individual. Dr. J. S. Wight, of Brooklyn, has made a valuable contribution to surgery relating to the comparative length of the inferior extremities in the same individual. His first published table comprised the measurements of sixty persons, of varied nationality, pursuits, and ages. In these sixty there were ten persons who presented a parity of length in the two legs, and fifty who showed a difference varying from one-fourth of an inch to one-and-three-eighths inches. The right leg was the longer in eighteen, and the left in thirty-two instances. A second table comprises forty-two measurements, and shows a parity of length in thirteen, and a difference in twenty-nine instances, the difference varying from one-fourth of an inch to one inch. In nine cases the right, and in twenty the left limb was the longer. Hamilton has corroborated these statements by his own researches. Gurson[1] has made some further investigations which tend to establish the correctness of these observations. He examined seventy skeletons of different sexes and ages, and belonging to different races. He found that among this number only in seven were the lower extremities of the same length, and among these the femur, tibia, and fibula were of the same length on both sides in two, while among the remaining five the tibia and fibula and the femur equalized the difference in length. In 54 per cent. of the cases the left extremity was longer than the right, the average difference being 4.8 mm., and the maximum difference 13 mm. The right extremity was longer than the left in 55.8 per cent., with an average difference in length of 3.3 mm., and a maximum of only 8.0 mm.

[1] Inequality in Length of the Lower Limbs. Journ. of Anat. and Phys., vol. xiii. p. 26.

These measurements not only prove that the lower limbs differ in length in a majority of cases examined, they likewise point out the importance of measuring the long bones separately for the sake of comparison when measurements are made for diagnostic purposes.

More or less shortening will take place in every case of fracture of the neck. M. Lisfranc and M. Lallemand[1] each have reported a case where the limb was longer. It is impossible to conceive in what manner the fracture could add to the length of the limb; and still the observations were undoubtedly correct, and an explanation can only be given by assuming that the amount of actual shortening was slight, and the patient's limbs were of unequal length. The amount of shortening depends on the degree of disjunction, the greater the longitudinal displacement, the greater the shortening. The shortening is always the direct result of muscular contraction or longitudinal displacement by impaction. In impacted fractures the maximum is reached at once, and the degree of shortening depends on the depth of penetration or mutual inter-penetration of the fractured ends. In cases of impaction the shortening remains stationary, as the fracture is not disturbed, and can only increase on the advent of inflammatory interstitial absorption. In fractures without the capsule, all resistance to muscular contraction is lost, and the maximum amount of shortening is reached as soon as the muscles have become contracted. If the capsule is intact, and remains attached to the lower fragment, shortening takes place gradually by stretching of the capsular ligament. In case the fragments are held in contact by the denticulated fractured surface shortening can only proceed after this medium of apposition has been removed by displacement of the bones, or after inflammatory osteo-porosis has removed the projecting spiculæ. This condition is often met with in intra-capsular fractures. The degree of shortening immediately after a fracture has been relied upon by some in determining the seat of fracture. Among surgeons there has been, however, such discrepancy of opinion in

[1] Dupuytren, Injuries and Diseases of Bones.

this respect, that no reliable deductions can be drawn from this circumstance in rendering a decision.

Sir Astley Cooper and Amesbury claim the greatest shortening for intra-capsular fractures, while Stanley, Earle, and R. W. Smith entertained an opposite opinion. Impaction and the integrity of the capsular ligament are such important factors in determining the amount of shortening and the time when it takes place, that these conditions must be carefully considered in estimating the value of shortening as a diagnostic aid.

5. *Change of Position of Trochanter Major.*—The trochanter major is displaced upward and backward, in proportion to the amount of shortening and eversion. When shortening has taken place, its upper border has passed beyond Nélaton's line. When the limb is rotated, it describes a smaller arc of a circle. It is less prominent when impaction has taken place, or when the lower fragment is not in apposition with the upper.

6. *Alteration of Motion.*—A false point of motion is always established in non-impacted fractures. Preternatural mobility is most marked if the fracture is not impacted and located outside of the capsule. It is probably in cases of this kind that Gerdy has been able to rotate the limb outward until the toes looked backward, and that Maisonneuve brought into requisition his test of hyper-extension. If the fracture is within the intact capsule, the latter will serve as a retentive measure, and limit the motion between the fractured bones. Dr. Levis has found that in non-impacted fractures the limb can be extended beyond its normal length. In case firm impaction has taken place, the neck has become shorter and thicker, conditions which necessarily impair the natural mobility of the hip-joint.

7. *Fascia Lata.*—Dr. Allis,[1] of Philadelphia, has added another symptom, which indicates fracture through the neck of the femur, namely, the existence of a relaxed condition of the fascia lata between the crest of the ilium and the trochanter major on the injured side, produced by the loss of resistance, which is furnished by the neck when not broken. As the presence of

[1] Mechanism of the Hip-Joint, Med. and Surg. Reporter, vol. xxxvi. p. 303.

this symptom depends on the dislocation of the lower fragment upward and inward, it is only met with when such changes have taken place. The standing position is the only one in which this test can be applied, as in the reclining position the muscles that make tense the fascia are relaxed.

Bezzi[1] has called attention to a sign which he considers as pathognomonic of fracture of the neck of the femur. In examining the space between the trochanter and the crista ilii, it will be found that while on the same side the muscles occupying this region (the tensor vaginæ femoris and the gluteus medius) are tense, and offer to the hand a considerable feeling of resistance, they present on the affected side a deep, well-marked depression, a flaccidity, and diminution of tension, from displacement upwards of their points of insertion. This sign appears under the same circumstances, and possesses the same significance as the one described by Dr. Allis.

I have intentionally omitted to mention crepitus as a symptom, as more harm than benefit has accrued from the efforts of the anxious surgeon to establish a positive diagnosis on the presence or absence of this sign. A careful study of the other symptoms will usually enable us to arrive at a correct conclusion without exposing the patient to the risks incident to the manipulations necessary for the purpose of eliciting this symptom.

XII. *Diagnosis.*

All manipulations, during the examination of a supposed fracture through the cervix femoris, should be performed with the utmost care and gentleness. The so-called "thorough examinations," the search for positive symptoms, has been the source of incalculable mischief. In many instances careless handling of the limb has resulted in disjunction of impacted fractures, or in tearing of periosteal or ligamentous bands, thus precluding most effectually possible union by bone or the forma-

[1] Centralbl. für Chirurgie, July 31, 1880.

tion of a short fibrous union. Years ago, Davis[1] entered his protest against such reckless examinations in the following emphatic manner: "Now, while we willingly concede the importance of a correct diagnosis in its bearings upon the successful treatment of any case, we hold that too much handling and manipulation of the limb in intra-capsular fracture is liable to eventuate in irreparable injury to the patient." Again: "When this connecting link of periosteum and capsular ligament is not severed by officious handling on the part of the surgeon, in his zealous, but often mischievous efforts to ascertain to the fullest extent the details of the injury, then we may hope for better results than have usually followed this accident."

Bryant's[2] caution is equally strong: "In fact the ordinary fracture of the base of the neck of the thigh-bone is primarily an impacted fracture, the impacted bone in some cases being loosened by a second fall, in others by excess of violence received in the original accident, and *in too many by the manipulations of the surgeon in his anxiety to make out the presence of a fracture by the detection of crepitus.* Indeed, this seeking for crepitus in cases of fracture is a practice fraught with danger."

I shall not allude to crepitus as a diagnostic sign; as a satisfactory diagnosis can usually be made without, by a careful consideration of the other symptoms. In every case of suspected fracture we should make careful search for evidences of senile osteo-porosis, and ascertain as nearly as possible the amount of force applied, and the direction in which it was applied. If the general appearances of the patient indicate the existence of senile osteo-porosis far advanced, the amount of force has been slight; and inflicted in the direction of the axis of the thigh-bone, it is more than probable that the fracture has occurred within the capsule. If the fracturing force has been greater, and applied transversely in the axis of the femoral neck, we have reason to expect that the fracture has taken place, at least partly, without the capsule. The sudden and complete loss of function of the limb after an injury to the hip in a person over fifty years of age

[1] Conservative Surgery, 1867, p. 23.

[2] Bryant's Practice of Surgery. Edited by J. B. Roberts, p. 842.

speaks strongly in favor of a fracture through the femoral neck. We can say with Hodgson: "If an elderly person, after a fall upon the hip, is unable to use the injured limb, it is very probable that a fracture of the neck of the femur has been sustained, and this is more likely to be the case, if, during the fall, no such great force has acted upon the greater trochanter as would be necessary to produce a contusion sufficiently severe to render the limb useless."

Aside from a general consideration of the case, the diagnosis will depend on the presence or absence of the two most important symptoms, shortening and eversion. Many of our best surgeons depend almost exclusively on accurate measurements in rendering a diagnosis. The amount of immediate shortening will vary according to the presence or absence of impaction, from a few lines to two inches. In impacted fractures the shortening is immediate, and remains stationary unless displacement takes place, or, during the reparative process, the femoral neck is shortened by interstitial absorption. The progressive shortening, a few days after the accident, is due to a loosening of the fragments which have been in mutual contact by denticulated projections, and to a gradual stretching of untorn portions of the capsular ligament. Mr. Bryant, in speaking of the utility of his "test-line," says: "Indeed, as a proof of its use, I may add that twenty-five consecutive cases of fracture of the neck of the thigh-bone, admitted into my wards to the end of 1877 (the average age of the patients being 74), left the hospital with union of the broken bones and useful limbs."

Dr. J. S. Wight, of Brooklyn, has written an exceedingly interesting and practical paper on diagnosis of fractures of the femoral neck, based on the report of twenty-one cases.[1] For the purpose of avoiding errors, which might accrue from asymmetry of the lower extremities, he directs that the following measurements should be taken: "1. Inside measurements from the superior anterior spines of the ilium to the lower ends of the internal

[1] The Diagnosis of Twenty-one Cases of Fracture of the Neck of the Femur. Proceedings of the Med. Soc. of the County of Kings, Oct. 1881.

malleoli. 2. Outside measurements from the superior anterior spines of the ilium to the lower ends of the external malleoli. 3. Measurements from the tops of the great trochanters to the lower ends of the external malleoli. 4. Measurements from the bases of the tibiæ to the lower ends of the internal malleoli. 5. Measurements from the superior anterior spines of the ilium to a line drawn transversely in front between the tops of the great trochanters."

The object of all these comparative measurements is to determine the possibility of original asymmetry of the two limbs, and to find out, as far as possible, if the injury to the hip has caused any shortening of the limb on the injured side, so that we can infer the *probability* of there being a fracture of the femoral neck. He gives the results of examination of twenty-one such fractures, where a diagnosis was made without eliciting crepitus. In eight of these cases there was probably impaction. The average shortening was $\frac{58}{100}$ of an inch, as shown by the inside and outside measurements. In no case of fracture of the femoral neck does he use force to find crepitus. He considers the other evidences of fracture as sufficient to come to a practical conclusion. His concluding statements contain so many practical and useful suggestions that I do not hesitate to quote them in full.

"1. Moving the outer fragment when it is in contact with the inner fragment, will generally carry the inner fragment with it, and there will be no crepitus. And, when there is impaction, ordinary manipulation will not cause crepitus to be felt. Yet crepitus may, at times, be felt when there is impaction of the neck of the femur. 2. Moving the outer fragment when it is not in contact with the inner fragment, of course will not give crepitus. 3. Hence unwarrantable force will be required in order to get crepitus in many cases of fracture of the neck of the femur, and more than this—*an impacted fracture of the neck of the femur may be broken up by severe manipulation, and a patient that would have had a useful limb may be quite completely disabled for life—for an impacted fracture of the neck of the femur is the best setting of the bony fragments that a surgeon can have.*

"In a suspected fracture of the neck of the femur, I examine

all the witnesses of fracture except crepitus, and if these witnesses agree substantially, I pronounce a verdict in favor of fracture of the neck of the femur; and if there is a doubt as to the correctness of such a verdict, I give the patient the benefit of that doubt by treating the case as if there was a fracture of the neck of the femur, and then the surgeon receives a benefit from the doubt. But if there is no fracture, the patient has had some days of needful rest, and has had a contused hip well treated."

The instrument recommended is an accurate steel tape-line, with feet and inches on one side, and meters and centimeters on the other side. This tape-line will not elongate under tension. It is superfluous to mention that the patient should be placed in the recumbent position, on an even surface, when the measurements are taken. It is to be hoped that the text-books of the future will say less of crepitus as a sign of fracture, and will substitute for it accurate methods of measurement.

Eversion of the limb is the next most reliable symptom. In impacted fractures the position of the limb depends on the direction of the fracturing force. If the force acts in the direction of the axis of the cervix, and is severe, causing implantation of the whole base of the neck into the trochanteric portion of the femur, the limb will retain its natural position. If the anterior wall is impacted by force applied against the outer and posterior aspect of the trochanter major, the limb will remain in a position of inward rotation. From the anterior obliquity of the neck, and the usual manner of falling (forwards and on the side), and the thinness of the compacta of the posterior concave surface of the neck, as compared with the anterior, we would naturally infer that posterior impaction takes place in the great majority of cases. This supposition has been abundantly verified by clinical observation. Impaction, then, is usually attended by eversion. If the fracture is located within the capsule, eversion frequently will increase for a few days, or weeks, after the accident, from the same causes which give rise to secondary shortening. In cases of posterior impaction, where the fragments

remain firmly implanted during the process of repair, eversion increases from the weight of the limb and the inflammatory absorption of the impacted fragments, permitting increased rotation outward of the lower fragment. The abnormal position of the trochanter major is also an important diagnostic sign. If we can exclude dislocation of the hip upward and backward, the application of Nélaton's test may decide the diagnosis. In cases of fracture of the neck of the femur, the upper border of the greater trochanter will be found above Nélaton's line, the distance corresponding with the amount of shortening. In non-impacted fractures, the false point of motion diminishes the axis of rotation which the greater trochanter describes in rotating the limb. This symptom is mentioned simply to be condemned, as the manipulations necessary to apply this test, like the search for crepitus, have done a great deal more harm than good. In doubtful cases, more particularly when dislocation is suspected, the patient should be carefully placed in the erect position, when the position of the limb and an examination of the contour of the hip, as well as an inspection of all the landmarks in that locality, will render material assistance in arriving at correct conclusions. In case of doubt, if we err at all, we should err on the safe side, and treat the case as one of fracture. Many cases which were in a condition most favorable for union by bone to take place, have been rendered hopeless by not following this rule. The surgeon should ever bear in mind that the most favorable cases present the least degree of deformity, and that in our anxiety to make a correct diagnosis we sacrifice all the conditions which are essential for obtaining bony union.

In response to my circular as to the possibility of bony union after impacted intra-capsular fracture, Prof. Alfred C. Post, of New York, after replying in the affirmative, kindly wrote: "But the difficulty in proving this proposition depends on two circumstances: I. The want of absolute demonstration that fracture has actually occurred. II. The want of opportunity to demonstrate by autopsy that bony union has actually occurred.

"It is a common thing for a person of advanced age to meet with an accident rendering him or her unable to stand or walk,

or to raise the affected limb from the bed. There is a certain amount of pain and lameness about the hip, with eversion of the toes, and a scarcely perceptible shortening of the limb. On careful examination, without using much force, neither crepitus nor abnormal motion can be detected.

"There is probable evidence, but not certain demonstration of impacted intra-capsular fracture. If the surgeon is contented with this imperfect diagnosis, he treats the case as one of fracture, and recovery takes place with a perfectly sound limb. But the proof of the fracture and reunion is incomplete. If the surgeon in his anxiety to obtain a perfect diagnosis moves the limb freely in all directions, he overcomes the impaction, rupturing the cervical ligament, demonstrates beyond all doubt the existence of the fracture, and effectually destroys all hope of reunion. For my part, I prefer an imperfect diagnosis for the surgeon, and a perfect limb for the patient, rather than a perfect diagnosis for the surgeon and a useless limb for the patient."

These remarks require no explanation. They are concise, plain, practical, and to the point. Unimpacted fractures of the neck of the femur seldom give rise to any difficulty in diagnosis; the symptoms attending them are so well marked that a correct conclusion can be reached without causing needless suffering or sacrificing important tissues in searching for any one particular positive sign. Fractures with impaction present the same symptoms in a minor degree, their presence can usually be recognized by a careful consideration of symptoms, the elucidation of which does not necessitate the disengagement of the fragments; and, finally, if we have reason to believe that a fracture with impaction exists, although the symptoms are not sufficiently well marked to warrant the diagnosis, it is our duty to initiate the treatment in accordance with such a supposition.

XIII. *Production of Callus.*

In assuming an affirmative position concerning the possibility of bony union after intra-capsular fractures, it becomes necessary, from a theoretical standpoint, to allude to the results of recent

researches on the production of callus. A brief historical review of this subject will be of interest to illustrate in how far the opinions of surgeons, regarding the mode of repair after fractures, have been influenced by the views they entertained as to the source from which callus is produced. Galen looked upon callus as a substance thrown out around the seat of fracture for the purpose of cementing the bones together, without, however, becoming changed into bone. Van Swieten claimed that the cement of Galen is transformed into bone. J. L. Petit compared the healing process of bone with the repair of soft tissues. Duhamel de Monceau attributed to the periosteum and endosteum the function of producing callus. Haller, and his prosector Detlef, believed that the periosteum took no part in the regeneration of bone, but that callus is derived from the fractured ends of the bones, more especially the myeloid tissue.

Dupuytren, from a clinical aspect, revived the theory of Duhamel, and at the same time attributed to the soft tissues around the seat of fracture bone-producing functions. He also introduced the terms provisional and definitive callus. He made the assertion that the definitive callus does not make its appearance until four to five months after the injury, and is not complete before eight to twelve months. Cruveilhier did not recognize the different kinds of callus described by his teacher, and ascribed its source to the lacerated soft parts surrounding the fractured bone-ends, the periosteum, connective tissue, muscles, tendon, etc.

Bransby B. Cooper defined callus as a plastic exudation from the inflamed ends of the broken bone. Lambron asserted that a broken bone can unite directly through the medium of an interfragmentary callus without the formation of a provisional callus. P. Flourens believed that the periosteum alone is capable of furnishing material for new bone. Subsequently, however, he modified his view, and made a distinction between the periosteal or permanent callus, and the temporary or muscular callus. August Voetsch speaks of callus as the product of traumatic periostitis. Rokitansky declares that callus is developed directly from bone and its connective tissue, including the periosteum.

Reinh. Hein,[1] who has studied this subject with great care by means of the microscope and experimentally, has come to the following conclusions: The regeneration of broken and resected bones commences, as a rule, from connective tissue. The process of regeneration is, at times, limited solely to the connective tissue of bone and periosteum, but in most cases the connective tissue of adjacent parts, more especially the muscles, contribute to it.

According to Virchow, callus is produced from connective tissue outside of the bone, and as well as from myeloid tissue in the interior of bone.

Preparatory to his studies on the production of callus, Hofmokl[2] has traced the histology of bone during fœtal life. During the development of bone, cartilage-cells are transformed into bone-cells. The primary marrow spaces are formed in the interior of cartilage-cells, which, with their contents, are transformed into marrow spaces. The normal development of callus appears, histologically, as a return of perfect bone into its primary stage, embryonal development. The periosteum, bone, and marrow are active in the production of callus. The neighboring soft tissues assist in the process of repair only in so far that they may become converted into bone. In point of importance, the callus-yielding tissues are arranged in the following order, periosteum, marrow, bone. The bone-cells take an essential part in the production of callus, since they become enlarged, multiply, and thus form marrow spaces with myeloid cells; changes which are observed very distinctly upon the surfaces of the ends of broken bone, on the periosteal, as well as on the medullary side. Ossification invariably begins from the margins of a medullary space.

Gegenbauer[3] takes the ground that bone is produced directly from connective tissue. Sharpey's fibres, if traced carefully,

[1] Ueber die Regen. gebrochener u. resec. Knochen. Virchow's Archiv f. Path. Anat. B. 15, 1858.

[2] Ueber Callusbildung. Virchow u. Hirsch, Jahresbericht, 1874, p. 294.

[3] Ueber die Bildung des Knochengewebes. 2 Mitth. Jenaische Zeitschr. f. Med. B. 3, S. 206.

always spring from a bony point between the Haversian canals, from which point they radiate towards both sides into the lamellar systems. The fibres form networks; and, at points of intersection, bone-cells are produced, and a deposit of lamellæ takes place around connective-tissue fibres. The intercellular substance is regarded by Gegenbauer as a product of secretion of cell elements, and not as a metamorphosis of cells, as was asserted by Waldeyer, who believed that the protoplasm of the cells is transformed in part or entire into basis substance.

Kassowitz,[1] in carefully studying the process of ossification, has come to the conclusion, that the deposit of earthy material in the fibrillary reticulum, as well as in the osteoblasts, is dependent on the condition of the circulation. The fact that the immediate neighborhood of the vessels does not ossify, and that the deposition of earthy material takes place in advance of the vessels, induced him to accept the theory that active circulation prevents the deposition of earthy material, while diminution of blood pressure favors ossification.

Rigal and Vignal's[2] experimental researches on the formation of callus have an important and direct bearing on the process of repair after fractures. Their practical deductions may be summarized as follows: If periosteum is exposed to a moderate degree of irritation, new bone is produced from the marrow beneath the point of irritation directly, without passing through the stage of cartilage. If irritation is increased by displacing the fragments and rubbing the soft parts, the result is cartilage beneath the periosteum, which is subsequently converted into bone. If the periosteum is completely destroyed by scraping the bone, the defect is repaired by a connective-tissue cicatrix, which somewhat resembles periosteum. If a circular piece of periosteum has been thus removed, and the bone is broken after cicatrization has been completed, perfect union is the result. If the cortical layer of bone is scraped away down to the medullary canal, the defect is replaced by myeloid callus. If the medullary

[1] Die Normale Ossification, etc. Wiener Med. Jahrb., 1879, S. 145.
[2] Virchow u. Hirsch's Jahresbericht, 1883, vol. i. p. 263.

canal is not opened, the process of regeneration is slower, as a considerable period of time will elapse until the resulting rarefying osteitis opens the Haversian canals sufficiently to furnish the required amount of cellular elements from the medullary tissue for the reparative process.

Mr. McNamara, in alluding to this subject, in his excellent work,[1] as applied to the neck of the femur within the capsule, says: "The ossification of the soft structures which grow from the medullary spaces of the broken bone is, in the human subject, a protracted process, and the tissues concerned are so delicate, that unless they are protected from injury by means of artificial splints, they seldom unite at all."

It is now generally conceded that the provisional or temporary callus is the product of the periosteal and para-periosteal tissues, while the definite or permanent callus is produced directly from the osteoid and myeloid tissues. The provisional callus is nature's splint, its only object being to immobilize the parts until the definitive callus firmly and permanently unites the fragments. The temporary callus is accidental, and appears earliest and most copiously where para-periosteal tissues are most abundant, and motion between the fragments greatest; the intermediate or permanent callus is produced later, and is most certain to take place in spongy bones. Fractures of the neck of the femur, partly within and partly without the capsule, unite with as much certainty as fractures in other localities in the usual way, by the formation of external and intermediate callus. In this variety of fractures an abundance of callus, sometimes bordering on deformity, designates the exact location of fracture. In intra-capsular fractures, as in fractures within any other joints, the conditions for the formation of external callus are unfavorable; hence, we find in all cases purporting to be bony union imperfect, if any, attempts in this direction. Anatomy, physiology, and experimental research, all tend to prove that in cases of intra-capsular fracture we have all the conditions present which are necessary for the production of intermediate callus, provided

[1] Diseases of Bones and Joints, 1881.

the fragments are kept in immediate contact for a sufficient length of time. The neck of the femur has been rendered vascular and porous by senile degeneration, and is supplied with an abundance of bone-producing myeloid tissue. The vessels in the red marrow, according to recent observation, are also admirably adapted for the purpose of establishing early and free collateral circulation. In 1869, Hoyer made the discovery that the small veins in the red marrow are without walls, their lumen being bounded by the parenchyma of the marrow. Most of the capillaries are also without walls. The small arteries of the marrow consist of a delicate tube of endothelium, and a single layer of muscular fibres. Rindfleisch corroborated these observations. From this peculiar structure of the vessels in marrow, it is easy to understand how readily the interrupted circulation could be re-established through immediate contact of the severed vessels, or by canalization through the medium of a blood-clot or mass of exudation material. That intermediate callus is thrown out in cases of intra-capsular fractures, where the fragments have not been kept in apposition, and bony union has failed to take place, is evident from examinations made of specimens where the broken surface of the upper fragment, and sometimes the connecting ligamentous band, presented well-marked spurs of hard compact bone; an appearance alluded to by many observers, but more particularly by Sir Astley Cooper and Mr. MacNamara.

XIV. *Can Loose Detached Pieces of Bone Produce Callus, and Aid in effecting Bony Union?*

It has been urged against the possibility of bony union after intra-capsular fractures that the upper fragment is not furnished with a sufficient vascular supply to maintain nutrition, much less to produce callus. Clinical and post-mortem evidence, however, tend to prove that in the great majority of cases the fragment retains its vitality, and in many instances where bony union has failed to take place, the fractured surface shows evidence of callus production. In such cases where the fracture was complete, and

the fibrous investment of the neck was completely torn across, the requisite vascular supply must have been furnished through the round ligament. If the upper fragment was not nourished from some source it would more frequently disappear by absorption, or suffer necrosis, and act as a foreign body, than has been actually observed at the bedside or in the post-mortem room. The establishment of collateral circulation through the ligamentum teres, in maintaining the vitality of the upper fragment after intra-capsular fractures, is, unquestionably, of more frequent occurrence and of greater importance than many are ready to admit.

Taking it, however, for granted that the ligamentum teres furnishes no vessels to the upper fragment, I shall, nevertheless, endeavor to show that in case of impaction it can retain its vitality, assist in the formation of callus, and enter into the production of bony union. It has been known for a long time that, in compound fractures, perfectly detached splinters remain inocuous, and assist in the production of bony callus without giving rise to any particular symptoms of irritation. John Hunter[1] expressed himself as follows on this subject: "Adhesion of the detached splinters also takes place, not only in those which are attached to the soft parts, but even such as are entirely loose. (This was shown in a thigh-bone in which one of the splinters had moved quite around on its axis, and adhered by its outer surface to the bone.) I never examined a compound fracture without finding some of those loose pieces, which shows they must be common. Their union must be similar to that in the transplanted teeth."

Ollier and Philip Walther inform us that they have seen the disk of bone separated by the crown of the trephine, and, entirely removed, reunite with the surrounding bone when replaced.

Prince,[2] in speaking of the drilling operation for ununited fractures, says: "When the operation results in the effusion of plastic lymph without suppuration, there are new centres of

[1] Works of John Hunter. Edited by James T. Palmer, vol. i. p. 502.

[2] Plastics and Orthopedics, 1871.

ossification in the chips of bone cut off by the drill. These are left in the track of the drill; some of them in the soft callus between the ends of the fragments. That these minute fragments of bone become parts of the living tissue is certain; for, if they did not, they would, by the offensive emanations of dead bone, excite suppuration and work their way to the exterior. The importance of these little fragments cut off by the drill, as centres of ossification, may have received too little attention."

Cases, where fragments of bone from the internal table of the skull were completely isolated and yet became attached to the surrounding bone by permanent callus, are reported by Samuel Thomas, Soemmering, Bernhard Beck, von Bergmann, H. Demme, Cluston, Richet, and Ziegler.

Lossen[1] has studied this subject in connection with comminuted fractures of the long bones, and has come to the conclusion that not all loose fragments necrose, but that many are incorporated in the callus and form part of the living ridge between the fractured ends. He is of the opinion that the vessels of the fragment, at some point, unite with other vessels in the lacerated district, thus establishing the circulation. In one of his illustrations may be seen a fragment, five centimeters long and one centimeter broad, completely isolated and denuded of its periosteum, which, with its wedge-shaped end, had been driven into the medullary cavity. The upper end was perfectly united with the bony mass filling the medullary cavity, and its lower end could be seen aside of the necrotic portion of the fractured bone. It can safely be assumed in this instance, that the vessels in the medullary cavity vascularized the fragment and preserved its vitality. Klebs gives a description of a similar specimen, and believes that the vitality of the medullary tissue and periosteum is sufficient to sustain the physiological activity of isolated fragments of bone under favorable circumstances, production of new bone taking place from the transplanted piece of bone.

[1] Kriegs-chirurg. Erfahrungen aus den Barackenlazarethen zu Mannheim, Heidelberg u. Karlsruhe, 1870, 1871. Deutsche Zeitschr. f. Chirurgie. Band II. S. 25.

v. Bergmann describes a specimen of comminuted fracture of the femur, the result of a gunshot wound during the Turko-Russian War, where a fragment, 7.2 ctms. long, 15 mm. broad, and 6 mm. thick, had become completely detached from the soft tissues, and had been forced into the medullary cavity, where it became firmly united with the fractured ends of the bone and the intervening bony callus.

Meek'ren made a series of experiments on animals for the purpose of establishing the fact that isolated fragments of bone devoid of periosteum would, under certain favorable conditions, retain their vitality, and were capable of forming an attachment to bone through the intervention of a bony callus. He removed by the trephine, from the skull of a dog, a disk of bone and replaced it. On the twenty-second day he found this disk firmly united by bony callus to the surrounding bone.

Flourens transplanted a piece of rib from a dog, under the periosteum of the tibia, of the same animal, and in due time found it united by bony callus. The well-known experiments of Ollier are familiar to every surgeon, but as he placed great importance on the preservation of the periosteum as an essential condition for success in bone transplantation, they are not of great importance for our purpose. The experiments of Kosmowski, to ascertain the exact mode of repair in cases of fracture of the skull, indicate that the reparative process in general, and the union of loose splinters of bone in particular, are accomplished by the osteo-genetic functions of the medullary tissue.

Of great practical importance are the experiments of Jakimowitsch.[1] The experiments were made exclusively on the long bones of dogs, and the vascular connections of the transplanted or replanted piece of bone were demonstrated by means of gelatine injections stained with Berlin blue. To insure success, he places great importance on securing accurate apposition and perfect immobilization of the fragment by stitching the periosteum or soft parts over it, and applying elastic pressure and a fixation splint of plaster of Paris. The operation was always

[1] Versuche über d. Wiederanheilen vollkommen getrennter Knochensplitter, Deutsche Zeitschr. f. Chir. B. XV.

done under strict antiseptic precautions. To prove that the detached bone had become a part and parcel of the living bone, some of the animals were fed on madder after the example of J. Wolff. This staining material is deposited during life in the new bone around the fragment in greatest abundance, while it also follows the new vessels into the transplanted piece. In almost all of the cases after death, the vessels of the limb operated upon were injected with gelatine stained with Berlin blue, which afforded an excellent opportunity to follow the course of the vessels into the transplanted or replanted piece of bone. In other instances the examination was made even more complete by decalcifying the bone and submitting it to a microscopical examination. The results of his experiments induced him to conclude that replantation and transplantation of isolated fragments of bone can be successfully performed if the detached piece retains its former relations to its immediate vicinity. Under such circumstances the piece of bone becomes a living part of the bone through the medium of the intermediate callus, and the re-establishment of vascular connections with the surrounding vessels.

Gurlt[1] describes and furnishes illustrations of two specimens of fracture of the femur, where a large fragment of the cortical layer near the centre of the shaft had become completely detached, and in one instance turned completely around, and yet they were found firmly attached by bony union. Both specimens are from the Museum of the Royal College of Surgeons of England, being numbered 108 and 454. He states further, that in comminuted fractures, where many loose fragments must exist, they furnish no obstacle to ready bony union. The fragments either take part in the formation of callus, or are imbedded in the mass, and eventually are removed by a slow process of absorption.

MacEwen[2] resorted to transplantation of small pieces of bone to restore extensive pathological defects, believing that the blood-clot between the fragments served as a medium through

[1] Op. cit.

[2] Virchow u. Hirsch, Jahresbericht, 1881, vol. ii. p. 332.

which the vascular connection between the detached bone and surrounding tissues is established. He operated successfully upon a case of necrosis of the humerus, with extensive loss of bone substance, by transplanting into a groove made in the bone numerous wedge-shaped pieces of bone derived from the tibiæ of six rickety children, the fragments being supplied with periosteum and marrow tissue. The bone grafts retained their vitality, united with, and grew with, the bone.

Prof. von Nussbaum has introduced transplantation of bone as a legitimate operation in surgery, for the purpose of supplying bone defect in cases of ununited fracture, and his success, as well as similar operations by several other German surgeons, certainly prove that the vitality of even compact bone is sustained by a minimum amount of blood-supply through a narrow strip of periosteum.

Spongy bone, containing an abundance of marrow tissue and a rich supply of bloodvessels, is endowed with a higher degree of vitality than compact bone; and is, consequently, better adapted to enter into union with surrounding tissues, in case it has become detached.

It has also been established by way of experiment, that in animals marrow can be transferred to different parts of the body, and if the operation is successful, the transplanted marrow will produce bone.

Baikow, Goujon, and Ollier were successful in their auto-transplantations of marrow, but failed when the tissue was transferred from one animal to another. The most extensive and reliable experiments on marrow transplantation have been made by P. Bruns.[1] He operated upon sixty chickens and six dogs. He failed repeatedly as long as he transplanted the marrow from animal to animal, but as soon as he limited his experiments to auto-transplantation, he succeeded in the great majority of cases. Of nineteen auto-transplantations, twelve proved suc-

[1] Ueber Transplantation von Knochenmark, Archiv. für Klinische Chirurgie. B. XXVI. S. 661.

cessful, three failed on account of suppurative inflammation following the operation, and in four the transplanted tissue was absorbed.

The operation consisted in removing cylindrical pieces of marrow from the femur or tibia, 2 to 3 ctms. in length, and transplanting them under the skin of the same animal. After the fourteenth day foci of ossification could be distinctly seen, which enlarged and became confluent after the twentieth to the twenty-fourth day. Ossification is preceded by an active proliferation of spindle-shaped cells.

The formation of bone takes place from the pre-existing osteo-blasts in marrow, an opinion which is also supported by Waldeyer. The yellow and red marrow were used in these experiments, and proved alike capable of producing bone.

The success attending bone and marrow transplantation constitutes a potent argument in favor not only of the *possibility* but *probability* of bony union after intra-capsular fractures, in the event that the fractured ends are in accurate and undisturbed apposition for the requisite length of time. The neck of the femur in a state of senile atrophy furnishes a number of favorable conditions for a speedy production of bony callus. It is very vascular, the compact tissue attenuated, the spongiosa exceedingly porous, and its meshes distended with an abundance of bone-producing myeloid tissue. If perfectly detached and denuded pieces of compact bone, and isolated masses of marrow, can be transferred to a distant part of the body, and when properly transplanted, they not only retain their vitality, but become vascular and produce bone, I can see no reason why the upper fragment in intra-capsular fractures, which is still retained in its natural location, should not manifest the same power of self-preservation and repair. In impacted fractures the bone-tissue, marrow, and lacerated vessels are brought in such immediate contact, that the reparative process is taxed only to its minimum extent in restoring the continuity of the bone. In these instances we have an example of bone and marrow transplantation under the most favorable conditions, and the reason it does not succeed

oftener, is simply because these favorable conditions, as a rule, do not exist, or are not allowed to exist, for a sufficient length of time.

XV. *Specimens of Bony Union after Intra-capsular Fracture.*

It is not my purpose to enter into a discussion of the many specimens for which bony union has been claimed by their possessors. Many of them have been the object of the most rigid criticism, at different times and at the hands of different writers. While careful and competent men have brought these specimens before the profession as typical examples of union by bone within the capsule, equally good observers have failed to see the evidence which justified these claims. I have tabulated only the cases reported by competent observers, and where the diagnosis was verified by a post-mortem examination.

No.	Name of Reporter.	Where mentioned or classified.	In whose possession.
1	Adams, R.	Todd's Cyclopedia. vol. ii. p. 813.	Adams.
2	Adler,	Am. Journ. Med. Sci., April, 1873.	Adler.
3	Bardeleben,	Lehrbuch d. Chir. B. ii. p. 477.	Goyrand.
4	Brulatour,	Med. Chir. Transactions, vol. xiii.	Brulatour.
5	Bryant,	Bryant's Surgery, p. 843.	Museum Guy's Hospital.
6	Callender,	St. Barthol. Hosp. Rep., vol. i. p. 154.	
7	Chassaignac,	Thèse inaugurale.	Van Houte.
8	Chelius,	Handb. d. Chir. B. i. p. 319.	Chelius.
9	Chelius,	Handb. d. Chir. B. i. p. 319.	Soemmering's collection.
10	Cushing,	Bigelow, The Hip, p. 133.	
11	Earle,	Practical Obser. in Surgery, 1823, p. 97.	
12	Fawcington,	Am. Journ. Med. Sci., vol. xv. p. 534.	Fawcington.
13	Fischer, H.	Personal communication.	Pathological Museum, Breslau.
14	Fischer, H.	Personal communication.	Ponfick.
15	Field,	Amesbury on Fractures.	Field
16	Geddings,	Am. Journ. Med. Sci., Jan. 1847.	Geddings.
17	Gurlt,	Knochen-brueche, vol. i. p. 308	Giessen Museum.
18	Hamilton,	Hamilton on Fractures, p. 407.	Hamilton.
19	Harris,	Am. Journ. Med. Sci., vol. xviii. p. 246.	Harris.
20	Holthouse,	Holmes's System of Surgery, vol. ii.	St. George's Hosp., Spec. No. 112.
21	Howship	Med. Chir. Transactions, vol. xiv.	Howship.
22	Hutchinson,	Illustr. Clin. Surgery, vol. ii. p. 8.	Leeds Hospital Museum.
23	Hutchinson,	"Museum Notes" of Jan. 23, 1870.	Museum of Trinity Coll. Dublin.
24	Jones,	Med. Chir. Transactions, vol. xxiv.	Jones.
25	Kocher,	Personal communication	Pathological Museum, Berne.
26	Kroenlein,	Personal communication.	Pathological Museum, Zürich.
27	Langstaff,	Med. Chir. Transactions, vol. xiii.	Langstaff.
28	Maas,	Personal communication.	Pathological Museum, Freiburg.
29	Malgaigne,	A Treatise on Fractures, 1859, p. 555.	Musee Dupuytren.
30	March.	Trans. Am. Med. Association, 1858.	Museum Albany College.
31	March,	Trans. Am. Med. Association, 1858.	Museum Albany College.
32	March.	Trans. Am Med. Association, 1858.	Museum Albany College.
33	Mussey,	Am. Journ. Med. Sci., 1857, p. 299.	Mussey.
34	Mussey,	Am. Journ. Med. Sci., 1857, p. 299.	Mussey.
35	Mussey,	Am. Journ. Med. Sci., 1857, p. 290.	Mussey.
36	Pope,	Hamilton on Fractures, p. 407.	
37	Post,	Personal communication.	Destroyed in fire of Univ. Med. Col.
38	Riedinger,	Studien über grund u. einkeilung der Schenkelhalsbrueche, 1874. Pl. xi.	Würtzburg Museum.
39	Roberts,	Personal communication.	Museum Pennsylvania Hospital.
40	Sands,	New York Med. Record, June 1, 1869.	Sands.
41	South,	Chelius Surgery by South, vol. i p. 621.	South.
42	South,	Quoted by Hamilton, Ed. 1871. p. 363.	Museum St. Barthol. Hosp.
43	Smith, H. H	Princ. and Prac. of Surg., vol. ii. p. 610.	Wister and Horner Museum.
44	Smith, H. H.	Princ. and Prac. of Surg., vol. ii. p. 610.	Smith.
45	Smith, R. W.	Dublin Journ. Med. Sci., Jan. 1873.	Museum Trinity College.
46	Smith, R. W.	Dublin Journ. Med. Sci., Jan. 1873.	Museum Trinity College.
47	Spalding,	Bost. Med. & Surg. Journ., Mar. 4, 1858.	Spalding.
48	Stanley,	Med Chir. Review, vol. xii. p. 170.	Stanley,
49	Swan,	On Diseases of Nerves, p. 304.	Swan.
50	Selden,	Trans. Virginia State Med. Soc., 1877.	Selden.
51	Selden,	Trans. Virginia State Med. Soc., 1877.	Selden.
52	Parker, W.	Johnson, Intracap. Fract., 1857, p. 28.	W. Parker.
53	Zeiss,	Hamilton, Fract. and Disl., 1880, p. 406.	Zeiss.
54	Zeiss,	Hamilton, Fract. and Disl., 1880, p. 406	Zeiss.

I will only give a description of a few undoubted specimens for the purpose of illustrating the alterations which take place in the femoral neck during the process of repair. R. Adams (Table, No. 1): "The round ligament was sound. The head and neck of the bone had lost their normal obliquity, and were directed nearly horizontally inwards; the cervix presented, both anteriorly and posteriorly, evidence of a transverse intra-capsular

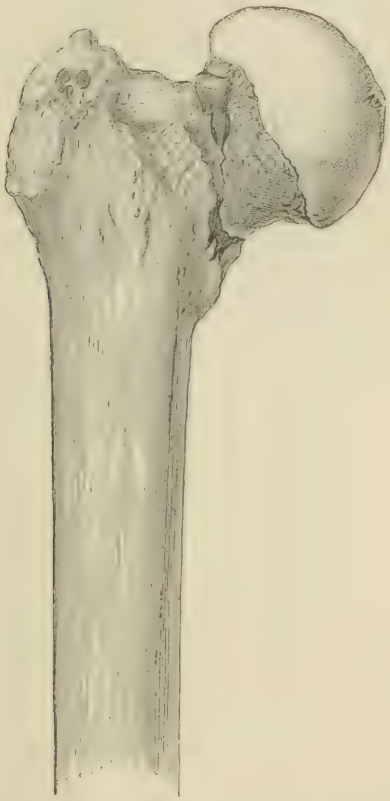
fracture having occurred; the globule-shaped head was closely approximated behind and below to the posterior inter-trochanteric line, and to the lesser trochanter, so that the neck seemed altogether lost, except anteriorly, where a very well-marked ridge of bone showed the seat of the displacement and of the union of the fragments. This ridge is evidently the upper extremity of the lower fragment of the cervix. The fracture of the neck posteriorly was found to have been closer to the corona of the head than anteriorly, and the fibro-synovial fold in the former situation remained unbroken. A section has been made of the bone through the head, neck, and trochanter; one portion has been subjected to maceration and boiling; and the bony union has been unaffected by these tests. Scarcely any portion of the neck can be said to have been left. The section shows the compact line which denotes the union of the fragments; the head and shaft seem to be mutually impacted into each other, and almost the whole of the cervix has been absorbed; the line of union is serrated, solid, and immovable; and the cells of the head and substance of the shaft seem to communicate freely in all places, except where the thin line of compact tissue here and there points out the seat of the welding together of the remaining portions of the head and neck of the femur."

As Mr. R. Adams, in his article, "Abnormal Conditions of the Hip-joint," in *Todd's Cyclopædia*, took the ground that bony union was impossible, and commented unfavorably on the cases which had been reported as cases of bony consolidation, it is evident that this case must have presented convincing proof in order to change his views on this subject. The value of this specimen is increased by a full clinical history of the case.

Chorley's specimen, described by Jonathan Hutchinson (Table, No. 22): "The bone, which supplied the illustration I now publish, is one of the many treasures of the Pathological Museum of the Leeds Hospital. The drawings were (by permission) made for me by Mr. Tuffen West, some years ago, at the time of the visit of the British Medical Association to Leeds. The specimen is the best example of union of an intra-capsular fracture with which I am acquainted, and, as it appears to be beyond all cavil,

I have great pleasure in endeavoring to secure for it a wider recognition. The drawings show so exactly the condition of the bone that it is scarcely necessary to describe them. (Figs. 9, 10, 11.) It will be seen that, whilst the transverse fracture is wholly

Fig. 9.



Anterior view of Bony Union after Fracture of Neck of Femur. (Hutchinson.)

within the capsule, and nowhere more than half an inch from the articular head, yet, that on the back of the cervix, some fragments have been detached, which pass much further out. It is worth notice, also, that in the section of the bone, the edge of the lower outer layer is seen to catch in the cancellous tissue of the articular fragment, thus constituting a degree of impaction,

which, no doubt, much favored fixation and union. The specimen was obtained by the late Mr. Chorley, formerly Surgeon

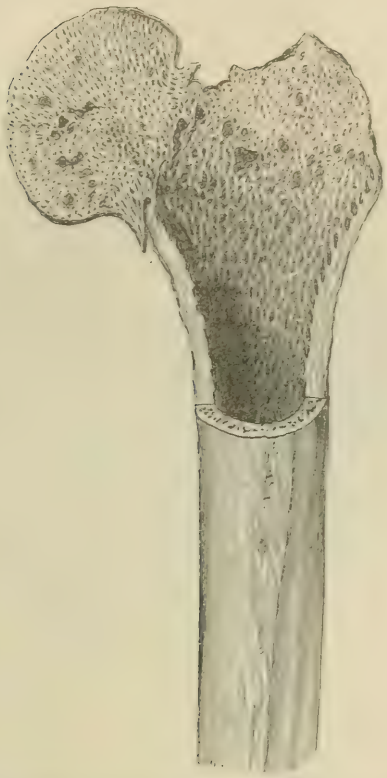
Fig. 10.

Posterior view of Bony Union after Fracture of Neck of Femur. (Hutchinson.)

to Leeds Infirmary, from the body of a gentleman, aged seventy, whom he had attended several years before his death, with the diagnosis of fracture of the neck of the thigh-bone. The treatment had been by very careful immobilization and long-continued confinement to bed. The recovery had been such that the patient had been able to walk well with a stick."

The well-known ability of Mr. Hutchinson is a sufficient guarantee for the genuineness of this specimen.

Fig. 11.



Vertical section showing Bony Union after Fracture of Neck of Femur. (Hutchinson.)

Riedinger's specimen (Table, No. 38): "The neck of the femur is considerably shortened, and the head inclines so far backward, that, superiorly, it comes almost completely in contact with the posterior inter-trochanteric line. From behind, only the cartilaginous surface of the head can be seen; downward, the neck is visible to the extent of 1 ctm. Above, the length of the neck is 1½ ctm. On the anterior surface, the well-marked denticulated line of fracture can be seen close to the head. Its length is 3 ctm. A longitudinal section of the upper portion of the femur, into an anterior and posterior half, discloses the line of fracture

in the loosely cancellated tissue of the spongiosa, and more clearly shows the impaction of the lower fragment into the head,

Fig. 12.

Anterior view of Bony Union after Intra-capsular Fracture. (Riedinger.)

which is especially well marked in the lower cortical portion of the neck (Adams's arch). The length of the implanted portion amounts to 2 ctm." (Figs. 12, 13, 14.)

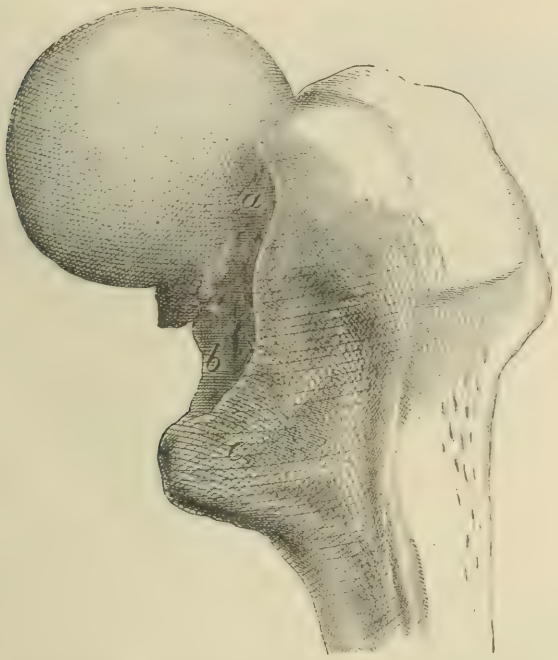
As Riedinger has made fractures of the neck of the femur a special study for many years, no one would for a moment doubt the correctness of his description, or the authenticity of this specimen.

Gurlt's specimen (Table, No. 17): "The fracture runs obliquely through the neck of the femur; in front it is three-fourths of an inch from the base of the neck, posteriorly a little less. The head of the bone is displaced somewhat backward and down-

ward, and is united by bone, although the line of fracture is still visible in places." (Figs. 15, 16.)

Gurlt's name occupies a position foremost among writers on fractures, present and past, and his decision admits of no appeal.

Fig. 13.



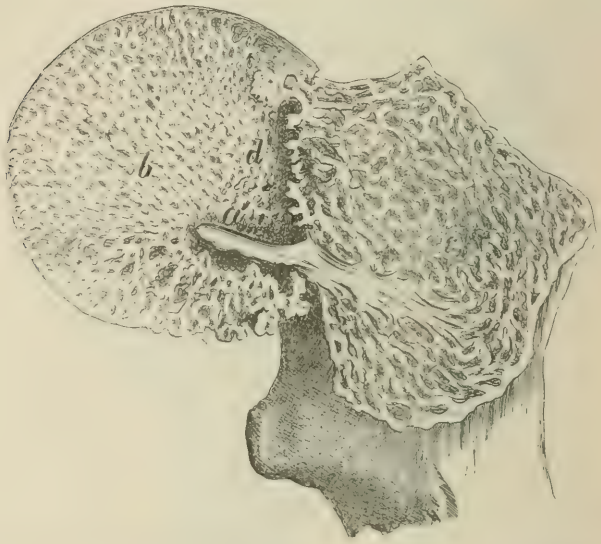
Posterior view of Bony Union after Intra-capsular Fracture. (Riedinger.)

To prove the validity of any specimen, it is necessary to examine for evidences which will warrant an affirmative answer to the following questions: 1st. Has the bone been fractured? 2d. Was the fracture within the capsular ligament? 3d. Has the fracture consolidated by bone?

The first question can only arise in specimens without a clinical history. Post-mortem specimens have been brought forward as instances of bony union, when the changes in the bone were due to other causes, as rickets or senile coxitis. In all cases of

interstitial absorption without fracture, the wasting of the neck takes place in a more symmetrical manner; the neck may become greatly shortened, and yielding to the vertical pressure, the head may descend to a level with the upper border of the

Fig. 14.



Vertical section showing Impaction with Bony Union after Intra-capsular Fracture. (Riedinger.)

trochanter major, but does not incline backward, as is generally the case when fracture has taken place. In senile coxitis the head is enlarged, and presents the characteristic deep depression for the round ligament, at the same time its upper and anterior surface is deprived of cartilage, and presents an eburnated appearance. (Fig. 17.) If rickets, or senile osteo-malacia, has been the cause of the deformity, the disease affects both joints simultaneously. An intra-capsular fracture always unites with some degree of deformity. Longitudinal sections of the specimens usually disclose the direction and extent of displacement of the fragments. From causes which have been previously enumerated, absorption of the neck is more extensive in the posterior portion of the

neck than in the anterior, permitting the head to approach the posterior inter-trochanteric ridge. If the fracture has been entirely within the capsule, little or no provisional callus is found over the seat of fracture, while in senile coxitis irregular bony

Fig. 15.

Fig. 16.

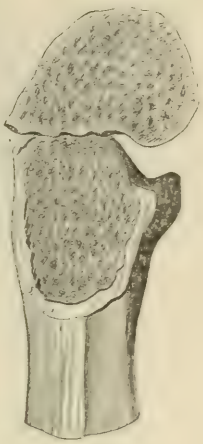


Fig. 15. Anterior view of Bony Union after Intra-capsular Fracture. (Gurlt.)
Fig. 16. Section through Neck showing Bony Union within Capsule. (Gurlt.)

masses are found over different portions of the neck. The writer on fracture of the neck of the femur in *Eulenburg's Encyclopædia* says: "If bony union takes place, the femoral neck disappears almost completely by absorption, the head coming nearly in contact with the trochanteric region. Little or no callus is found upon the surface of the neck."

Bardeleben[1] indicates the following appearances as characteristic of union by bone after fracture within the capsule: "If it can be ascertained with certainty that a fracture had occurred during life, and on post-mortem examination we find a bone cicatrix, that is a disk of dense bone through the intra-capsular portion of the neck, and there are no other evidences of syno-

[1] Lehrbuch d. Chirurgie, 1871, B. II. S. 473.

vitis or osteitis, then we are justified in claiming for such a case that a fracture within the capsule has united by bone."

Fig. 17.

Appearances of Head and Neck of Femur in Senile Coxitis. (Richardson.)

Erichsen[1] remarks: "When bony union has taken place, the head will usually be found somewhat twisted round, in such a

[1] The Science and Art of Surgery, 1869.

way that it looks toward the lesser trochanter, owing to the eversion that has taken place in the lower fragment."

Gurlt[1] states: Absorption of the fragments takes place exclusively in fractures involving joints, and proceeds hand in hand with the process of repair. In some joints, as in the hip-joint, it may be so extensive that almost the entire neck is absorbed. This is more likely to be the case if the fracture is within the capsule. In such cases the head of the bone may be very near the great trochanter at the base of the cervix femoris. The cause of this absorption is not known.

The characteristic deformity presented by specimens of bony union of fracture through the neck of the femur corresponds to the direction of the displacing forces—shortening and eversion. The cause of the primary displacement is the fracturing force itself. The secondary displacement takes place upon the accession of inflammatory osteo-porosis, and is the result of softening and absorption of the bone, muscular contraction, and gravitation.

Exacting critics have questioned the validity of many specimens of bony union on the ground that the fracture was not purely intra-capsular. Indeed, this argument has been the main support of all modern believers in non-union. In all specimens of bony union, the point of attachment of the posterior portion of the capsular ligament is changed; instead of being inserted near the middle of the femoral neck, it is found attached at or near the posterior inter-trochanteric line, and on this account it has been asserted that the fracture extended beyond the capsular ligament. I believe, however, that this alteration in the attachment of the capsule admits of a more satisfactory explanation. All fractures are followed by inflammatory osteo-porosis in the ends of the broken bones, and this is especially well marked in the articular extremities of the long bones. During an inflammation of this kind, the periosteal covering of the bone is loosened, and readily changes its relative position to the bone during the process of interstitial absorption, and carries with it the capsular

[1] Handbuch d. Lehre v. d. Knochenbruechen, 1862, vol. i.

ligament with which it is intimately connected. Interstitial absorption precedes and attends the production of callus, and is most active in that portion of the bone supplied with the greatest number of bloodvessels. The upper fragment being scantily supplied with bloodvessels, absorption, if it takes place at all, occurs at a later date, and progresses very slowly; while the reverse is the case in the lower fragment. The point of attachment of the capsular ligament is no indication as to the seat of fracture, as almost the entire femoral neck may disappear by absorption, and the capsule approaches the trochanteric region in proportion to the amount of bone absorbed. A more important sign is the presence or absence of new bone upon the outside of the capsule. In intra-capsular fractures little or no external callus is produced within or without the capsule; while extra-capsular fractures, from obvious anatomical reasons, yield an abundance of exuberant callus, part of which at least remains permanently. The last test is to ascertain the nature of the connecting medium. This can be done by submitting the specimen to a microscopical examination, or to the boiling process. In the first test the tissues at the seat of fracture will show the histological elements of true bone in all genuine specimens. The boiling process will destroy the ligamentous union between the fragments in all doubtful cases; and is, therefore, the simplest and most certain method to demonstrate the restoration of the continuity of the broken bone.

In recapitulation, it may be stated, that the validity of a specimen is established whenever the clinical history has revealed the existence of fracture during life, and the post-mortem examination has demonstrated that the fracture has been within the capsule, and that the union is by bone.

XVI. *Non-union after Intra-capsular Fractures.*

Sir Astley Cooper enumerates the causes of non-union under the following heads: 1. Want of proper apposition of the bones. 2. Want of pressure of one extremity of the broken neck upon the other, even though the limb preserved its length, and the

fractured parts are consequently not much displaced. 3. Absence of nutrition in the head of the thigh-bone. 4. Atrophy of bone.

The first cause can only apply to non-impacted fractures, where treatment has failed to keep the fractured ends in immediate and uninterrupted contact for a sufficient length of time for union by bone to take place, and as such constitutes the principal, if not the only, cause of non-union. There is no other fracture where immobilization is so difficult to accomplish. Every movement of the body disturbs the fractured ends. No apparatus yet devised has answered the first and principal indication in the treatment of all fractures, namely, to secure immobility and permanent coaptation.

Colles,[1] who fully endorses the views of Sir Astley Cooper on the subject of fractures within the capsule, in speaking of the causes of non-union, remarks: "However this may be, I think the difficulty of keeping the parts motionless on each other would be sufficient of itself to account for it." Gurlt,[2] who has studied the process of repair in fractures with the most assiduous care, says: "There is no specific tendency to non-union in any form of fracture. If the ends of the broken bones can be kept in accurate apposition, union by bone will take place." As illustrations of this statement, he mentions the following fractures: Neck of femur, patella, coronoid process of inferior maxilla, coracoid process of scapula, olecranon, coronoid process of ulna, trochanter major, tuberosity of calcaneus, spinous processes of vertebræ, and some of the sharp prominences of the pelvic bones.

The second cause of non-union. Want of pressure of one fragment upon the other implies a want of apposition, expressed in other words. Dupuytren and Brainard were of the opinion that oblique resulted more frequently in non-union than transverse fractures, and the former applied this rule to fractures of the neck of the femur. Experience has shown that of all frac-

[1] Lectures on Surgery, 1845.

[2] Handbuch der Lehre von der Knochenbruechen, 1862, vol. i.

tures within the capsule, none are so prone to result in non-union as transverse fractures through the narrowest portion of the neck. Lateral pressure applied over the trochanter major is an important measure for obtaining union by bone, but this desirable result does not follow from the fact that pressure is made, but simply because by the pressure coaptation and immobilization are effected.

Deficient vascular supply of the upper fragment is prominently mentioned by almost every author against the probability of union by bone. On the other hand, it is generally submitted that fractures of the anatomical neck of the humerus unite by bone, and that completely isolated pieces of bone, when properly replanted or transplanted, retain their vitality and physiological properties. It is also well known that traumatic or pathological epiphyseolysis may be repaired by bony callus. Why should the upper fragment in intra-capsular fractures, with at least a doubtful supply of blood through the round ligament, make an exception to this general rule? Simply because, in this instance, coaptation, without impaction, is next to impossible, with the present method of treatment. On this point MacNamara[1] makes this statement: "I hardly think the non-union between the ends of the bone in instances of intra-capsular fracture of the neck of the femur is most frequently due to the insufficient blood-supply of the head of the bone; otherwise we should more commonly meet with examples, after fractures of this kind, in which the head of the bone had become absorbed; but, as you will see in the specimen I now show you, the cancellated tissue of the head of the bone is supplied with blood through vessels passing along the round ligament and through the fibrous structure uniting it with the trochanter major."

The fractured head of the humerus, deprived of all vascular supply, unites by bone like any other fracture, because the anatomical relations about the seat of fracture are such that coaptation is maintained without difficulty, and fractures within the capsule of the hip-joint will follow the same rule as soon as

[1] Diseases of Bones and Joints, 1881.

the surgeon can successfully combat the obstacles which cause displacement. The last cause, atrophy of bone, is the weakest argument in favor of non-union. Clinical experience furnishes abundant proof that in persons suffering from fragilitas ossium fractures not only unite, but unite very promptly. Mr. Holmes, in his System of Surgery, quotes from Gibson the case of a youth of nineteen, who had twenty-four fractures, and from Esquirol another with as many as two hundred fractures. Earle records a case of eight fractures in a child of ten years, and Flemming observed a case where a person suffered fifty-three fractures between the ages of one-and-a-half and twenty-five years. In all of these cases union took place rapidly. Gurlt reports a large number of similar cases. He states very distinctly that *old age does not retard the process of union, as has been erroneously supposed; the reparative process is the same as during adult life.*[1]

I believe non-union is more frequently, observed in young, robust persons than in old people. I have seen a fracture of the femur, at the junction of the middle with the upper third, in an old decrepit man, suffering at the same time from locomotor ataxia, unite firmly by bone in less than six weeks. Fracture of the lower end of the radius is common after middle life, invariably unites, and in a remarkably short time. Senile osteo-porosis is a condition of bone favorable to the production of intermediate callus. Atrophy of bone facilitates inflammatory osteo-porosis, an event which always precedes the formation of callus. Some authors mention still other causes for non-union, as the presence of synovia and the absence of a nidus for the formative material. Both of these conditions simply remind us that the bones are not in apposition, otherwise they have no significance in preventing union by bone. From this short review we are not only justified, but warranted, in asserting that the only cause for the non-union in cases of intra-capsular fracture is to be found in our inability to maintain perfect coaptation

[1] Italics my own.

and immobilization of the fragments during the time required for bony union to take place.

XVII. *Bony Union after Intra-capsular Fractures.*

In a circular letter, I addressed to prominent surgeons in this country, England, France, Germany, and Switzerland, for the purpose of ascertaining the prevailing opinion on the subject of bony union after intra-capsular fractures, I asked this question: "In your opinion does bony union ever occur after impacted intra-capsular fracture of the neck of the femur, and under what circumstances?

To this question I received fifty direct replies. The opinions were divided as follows: Yes, 27; no, 18; doubtful, 5. It is a significant fact that the replies from professors of surgery in German universities, 5 in number, were without exception, in the affirmative, while the greatest diversity of opinion appeared to exist in our own country; at least fifty per cent. of the correspondents replied with an emphatic "No." I believe the answers received reflect correctly the sentiments of the entire profession on this point. If we add the five doubtful correspondents to the eighteen negative, we have nearly 50 per cent. who do not believe it possible for bony union to take place within the capsule even under the most favorable circumstances.

I consulted the text-books and monographs on this subject with about the same result. It would then appear that nearly one-half of the profession still doubt the possibility of union by bone in cases of intra-capsular fractures.

Having shown that there are no anatomical and physiological impossibilities present to prevent osseous union after intra-capsular fractures, and having referred to a number of reliable and well-authenticated cases of this kind, I will quote the opinion on this subject of a few recognized authorities.

Sir Astley Cooper, the originator of the controversy on this subject, and who is always quoted as authority on the negative side of this question, never denied the possibility of union by

bone, as is evident from what he says on page 137 of his work:[1] "I have only met with one in which a bony union had taken place, or which did not admit of a motion of one bone upon the other. To deny the possibility of this union (bony union), and to maintain that no exception to the general rule can take place, would be presumptuous, especially when we consider the varieties of direction in which a fracture may occur, and the degree of violence by which it may have been produced." He enumerates a number of conditions which would maintain permanent apposition, and then proceeds: "Such a favorable combination of circumstances is of very rare occurrence." At the time this was written the process of repair in bone was but imperfectly understood, and the occurrence of impaction within the capsule was either unknown, or its importance as an essential element for bony union was not appreciated.

Heister,[2] nearly a century and a half ago, after explaining that the frequency of non-union in cases of fractures of the femoral neck was owing to the difficulty of keeping the broken ends of the bone in apposition, makes the following statement: "If an instrument could be invented which would keep such a limb so extended that during the cure, or at least during the first two or three weeks, it could be kept as long as the healthy one, there would be hope that the fracture could be cured more satisfactorily than has been the case heretofore." Since we have learned that the production of the intermediate callus requires months instead of weeks, Heister would have to modify his statement by greatly extending the period of time required for maintaining apposition.

Desault,[3] in combating the popular idea of insufficient blood supply as a cause of non-union, states: "The head of the bone separated from the soft parts, and attached to the acetabulum by the round ligament, receives a sufficiency of nutriment to enable it to live in that cavity; for there is no instance of its having suffered mortification in consequence of a fracture. Why, then, should it not partake of the properties of life, and particularly of

[1] Fractures and Dislocations. [2] Chirurgie, 1747, p. 193.

[3] A Treatise on Fractures, etc. Edited by X. Bichat; translated by Chas. Caldwell, 1817.

the faculty of reunion when placed in regular apposition with the body of the bone?"

The following quotation is from Syme:[1] "But none of the arguments, which have been adduced to prove the *impossibility* of osseous junction, seems to be conclusive, and though the small extent and mobility of the broken surfaces, the absence of vascular tissues surrounding the fracture, and, perhaps, also the presence of synovial fluid, may render the cure very difficult, it ought still to be regarded as a possible occurrence."

Richter[2] claimed that bony union could take place in impacted fractures, or where, by careful treatment, apposition and retention were fully accomplished. He evidently was impressed with the importance of the bone-producing function of the periosteum, as he advanced the theory, that, in fractures of the neck with complete rupture of the periosteum under favorable conditions, bridges could be thrown across the line of fracture from one membrane to the other, from which bone could be produced.

Dupuytren,[3] in criticizing the treatment followed by the English surgeons, and alluding to the secondary displacements following the too early removal of retaining apparatus, gives the following advice: "But, if these surgeons had adopted the practice of the *Hôtel-Dieu*, in keeping their patients in bed for eighty or even a hundred days, they would have been convinced of the practicability of reunion and complete cure without deformity." And again: "I can only say, for my part, that, if the specimens at the *Hôtel-Dieu* are insufficient to satisfy any one who may take the trouble to examine them, I am at a loss to know what amount of evidence such sceptics would require. For my part, I regard the osseous union of intra-capsular fracture as demonstrated and placed beyond doubt."

Malgaigne[4] is a firm exponent of Sir Astley Cooper's teach-

[1] The Principles of Surgery, 1832, p. 261.

[2] Lehrbuch von den Bruechen und Verrenkungen der Knochen, 1833.

[3] On the Injuries and Diseases of Bones. Edited and translated by F. Le Gros Clark, 1847.

[4] A Treatise on Fractures. Translated from the French by John H. Packard, 1859.

ings, and yet, after the most critical examination of specimens for which bony union was claimed, he is forced to acknowledge that three of them were genuine. He says: "When a fracture unites, the fragments do not undergo such enormous losses of substance as we should be forced to admit in the neck of the femur; and in Swain's case, which Sir Astley Cooper himself acknowledged as an instance of bony union, the neck of bone had not changed its form. It was so also in Stanley's case; and, lastly, one femur (No. 188), in the Musée Dupuytren, has lost nothing, either in form or volume, except as the result of very trifling displacement. I admit that these three examples demonstrate quite positively the existence of consolidation; but I cannot say the same of the rest." Loss of substance and change of direction of the neck can no longer be regarded as evidence against the existence of bony union, as they only indicate the presence of impaction followed by interstitial absorption, the consequence of inflammatory osteo-porosis.

Nathan R. Smith,[1] in recommending his anterior splint in the treatment of fractures of the neck of the femur, expresses his convictions as follows: "This apparatus, with slight modifications, is applicable to all fractures of the femur. To none is it more appropriate, and in none has it accomplished more satisfactory results, than in fractures of the cervix, the events of which are so justly regarded as an opprobrium of surgery. So uniformly has non-union and deformity resulted, that eminent surgeons have denied that bony continuity is ever restored within the capsule. We hope to show that these results are rather the consequence of insufficient treatment, than defect in the reparative power of nature."

H. H. Smith[2] advocates the possibility of bony union in the following language: "That osseous union has been seen, cannot reasonably be doubted, and from a careful analysis of the seat of fracture in these cases, I think it is evident that there are a com-

[1] Treatment of Fractures of the Lower Extremity, by the use of the Anterior Suspensory Apparatus, 1867.

[2] The Principles and Practice of Surgery, 1863.

paratively limited number of cases in which osseous union does occur; and I suggest that, as a general rule, based on observation, it will be found that the nearer a fracture is situated to the head of the bone, or, in other words, the shorter the upper fragment, the greater will be the possibility of osseous union; because the shorter the upper fragment, the greater the chance that the vessels which supply it with blood through the round ligament will be able to furnish it with an amount of material sufficient to enable osseous union to take place by a deposit of bone from the Haversian canals."

Samuel Solly[1] writes: "If you can diagnose that the fracture is an impacted fracture of the cervix, then you may with tolerable confidence predict complete union and a sound limb. I have shown by reference to the preparations in the College of Surgeons' Museum, and also in our own, that fractures of the cervix within the capsule will unite, though not so frequently as those without."

Chelius[2] claims that bony union may have been observed less frequently in England than on the Continent, on account of neglected treatment in cases diagnosticated as intra-capsular fractures.

Erichsen,[3] in discussing this subject, remarks: "In some cases, however, bony union takes place. This may happen when the cervical ligament remains intact, or when the fracture is impacted."

Holthouse[4] says: "Bony union in this fracture (intra-capsular) is rare, and by some has been considered impossible; but a sufficient number of undoubted cases have now been brought to light, both in Europe and America, to place the fact beyond a doubt."

Agnew,[5] in speaking of Astley Cooper's method of treatment of intra-capsular fractures, remarks: "There have been recorded

[1] On Fractures of the Neck of the Thigh-bone, London Lancet, 1867.
[2] Handbuch der Chirurgie, B. I. S. 119.
[3] The Science and Art of Surgery, 1869.
[4] Holmes's System of Surgery, 1875, vol. ii. p. 846.
[5] Principles and Practice of Surgery, 1878, vol. i. p. 938.

a sufficient number of cases of bony union, after what was believed to be intra-capsular fracture, to justify a hope that some of the cases encountered by the surgeon may have a similar termination."

Gant[1] expresses a similar opinion: "Bony union at one time, and for many years, thought never to take place, does assuredly in some rare cases; but only, it would seem, when the capsular ligament remains entire, or the fragments are impacted, whereby a due supply of blood can be speedily established."

Bryant[2] makes use of the following language: "In the impacted fractures union ought to be looked for if the broken fragments are left alone, and not loosened by a careless and too curious manipulation. In the purely intra-capsular fractures, union may take place, osseous in many cases, fibrous in more."

MacNamara[3] affirms: "I believe, if you can keep the parts at rest, in many cases of intra-capsular fractures, union of the ends of the bones will occur."

Koenig[4] realizes the importance of impaction in the reparative process, as may be seen from his statement that intra-capsular fractures heal less frequently by osseous union than extra-capsular fractures, because they are less frequently impacted.

Hueter,[5] who classifies fractures of the neck of the femur into those with and without impaction, regardless of the attachment of the capsular ligament, lays it down as a rule that impacted fractures usually unite by bony union.

Stimson,[6] in discussing this subject, advances the following as one of his arguments in favor of the possibility of bony union: "Even if we disregard all existing specimens of alleged bony union, the possibility of such union must, I think, be admitted, because of the demonstrated fact that the head preserves its vitality, and has shown its ability to produce granulations and

[1] The Science and Art of Surgery, 1878, p. 646.
[2] Principles of Surgery, edited by J. B. Roberts, 1881.
[3] Diseases of Bones and Joints, 1881.
[4] Lehrbuch der Speciellen Chirurgie, 1879, B. II. S. 857.
[5] Grundriss der Chirurgie, 1882, B. II. S. 884.
[6] A Treatise on Fractures, 1883, p. 503.

bone; the former proved by the examples of fibrous union, the latter by eburnation or condensation of its spongy tissue."

I will close the list of witnesses who testify to the possibility of bony union after intra-capsular fractures, by quoting the last sentences of Jonathan Hutchinson's description of the specimen in the Pathological Museum of Leeds Hospital. "This specimen is alluded to by Malgaigne and Hamilton, as if it were of doubtful validity; but neither of them had probably seen it. I cannot but hope that the publication of these life-size drawings of the bone will set at rest all scepticism as to the possible union of intra-capsular fractures. I trust, also, that it may lead to greater hopefulness in the treatment of these accidents, and thus to more systematic care in securing coaptation."

With such an array of unprejudiced, honest, and conscientious witnesses before us, who unanimously and most positively testify that union by bone can, and not infrequently does, take place, we are no longer warranted in denying its possibility. The number of well-authenticated specimens has been gradually increasing, and the knowledge derived from clinical observation and experimental investigations on this subject during the last few years, can leave no further doubt pertaining to the production of bony callus in intra-capsular fractures. In the interest of science, and for the benefit of the patients, this controversy ought and must be decided in favor of the affirmative, and then the profession will be prepared to seek for measures which will secure better results.

XVIII. *Treatment.*

In no other fracture are the indications for successful treatment so difficult to meet as in fracture of the neck of the femur. Every unprejudiced surgeon is forced to admit, that the usual bad result in these cases is owing more to the insufficiency of the treatment employed than to the anatomico-pathological conditions of the broken bone. The causes of non-union are not to be found in the broken bone, but in the difficulties encountered in the treatment. All the various methods of treatment, sug-

gested and practised, have failed in securing perfect coaptation and uninterrupted immobilization. In all intra-capsular fractures union is effected by the production of an intermediate callus, from the broken surfaces; nature's splint, the external callus, for well-known anatomical reasons, is always wanting, hence, the surgeon's splint has a more important and prolonged application than in fractures in other localities. The time required for bony union to take place in fractures of the femoral neck is an unusually long one. Gurlt[1] fixes the time at from 56 to 207 days, and the average duration at 84 days.

Dupuytren estimates the time at from 100 to 120 days, and states, that it has been customary at the *Hôtel-Dieu* to keep these patients in bed at the hospital for 80 to 100 days. There can be no doubt that many cases, which promised well from the beginning, terminated unfavorably by abandoning the treatment too early. To prevent secondary displacements, the retentive measures should not be removed for at least 80 to 100 days. In deciding upon a course of treatment to be pursued it is important to make a distinction between impacted and non-impacted fractures. In impacted fractures the fragments have been placed in the best possible condition for bony union to take place, and the object of treatment consists simply in maintaining the mutual penetration until the reparative process is completed, and the continuity of the bone restored. The surgeon must be satisfied with securing consolidation of the broken bone in the position in which it has been placed by the accident. Any attempt to correct the deformity is unjustifiable, and would necessarily result in loosening of the impaction, an event which would almost to a certainty be followed by non-union. Permanent fixation of an impacted fracture is necessary for the following reasons:—

1. It maintains the impaction. 2. It prevents secondary shortening and eversion during the stage of inflammatory osteoporosis. 3. By keeping the injured parts at rest, it serves as a preventive measure against the accession of arthritis and paraarthritis. 4. It enables the patient to leave the bed before com-

[1] Knochenbrueche, vol. i. p. 331.

plete consolidation of the fracture has taken place. Extension is always contra-indicated in these cases, as it certainly can do no good, and may result in irreparable damage by loosening the impaction. The best dressing to accomplish permanent fixation is a plaster-of-Paris bandage. To insure complete immobility of the hip-joint, the bandage must include the injured limb from the toes upwards, the entire pelvis, and the sound limb from the pelvis to at least as far as the knee. For the purpose of greater durability and security of the dressing, a tin or wood splint can be incorporated in the plaster bandage. In the application of this bandage it is necessary to protect all prominent bony projections, more especially the trochanter major over the affected side, with salicylated cotton, to guard against excoriations; a flannel bandage should be applied next the skin. During the application of the bandages, and until the plaster sets, it is necessary to place the patient on a pelvic rest, such as is described by Bardeleben. During the setting of the plaster, it is important to make lateral pressure over both the greater trochanters, in order to secure firm support to the broken bone.

With such a dressing, the patient can be moved without fear of disturbing the fracture, and in a few days he can leave the bed, and in a few weeks can walk on crutches, if deemed necessary for the purpose of preventing complications. Unless indications arise, it is advisable not to disturb the dressing until osseous union has become sufficiently firm to support the fragments. It is particularly dangerous to change the dressing from the third to the fifth week, as during this time the inflammatory osteoporosis has a tendency to loosen the fragments. I am satisfied that a dressing of this kind is vastly superior to any splint in affording comfort to the patient and securing the best attainable result.

In the treatment of non-impacted fractures the same principles should govern us as in the impacted variety. In this class of fracture, however, another important indication arises, namely, to effect coaptation of the fractured ends; at the same time retention is more difficult to accomplish. The nearer we can imitate impaction, the better the prospects for a favorable result.

If we could keep the broken surfaces in perfect coaptation, and maintain retention and immobility, these fractures would heal in the same way as impacted fractures. That these indications have not been fulfilled by the usual treatment with different splints, extension by weight and pulley, and pelvic belt, nobody can deny. Even extra-capsular fractures have healed, as a rule, with so much shortening as to cripple the patients for life, while the results after intra-capsular fractures have almost universally been so bad, that many of the most distinguished surgeons have abandoned all active measures, limiting their treatment exclusively to palliation. Prominent among the advocates of the expectant treatment in intra-capsular fractures, I will mention Sir Astley Cooper, Velpeau, Langlet, and Lavacherie. That the views of many surgeons on this point have undergone no material change since Sir Astley Cooper's time is apparent from more than one recent work on surgery. I quote verbally from Gant's surgery, page 647: "No bony union taking place, as a rule, in intra-capsular fractures of the neck of the femur, it will generally be useless to adjust the fracture and apply any retentive apparatus with a view to such union; and the more so, in proportion to the years of the patient."

If the results attending the different methods of treatment have been so bad as to induce men of the highest professional attainments to abandon all active treatment as useless, the question naturally arises, are there any other means which are better adopted to accomplish the desired result? The question as to possible bony union after intra-capsular fracture, in the light of recent researches, has been decided in the affirmative, and a more practical question arises, how can it be obtained? By what means can we keep the fragments in mutual coaptation during the process of repair? I would suggest the following points in the treatment: 1. Immediate reduction and coaptation of the fracture under the influence of an anæsthetic. 2. Fixation with a plaster-of-Paris splint. 3. Lateral pressure. 4. Direct fixation of fragments by bone-pegs.

I. Extension by means of the weight and pulley overcomes the shortening only gradually, and seldom completely, at the

same time it necessitates the recumbent position for a long time, and thus exposes the patient to all the risks and inconveniences incident to such position. If the patient is placed thoroughly under the influence of an anæsthetic, muscular action is temporarily annihilated, and the limb can be extended at once to its natural length, while coaptation can be effected at the same time. The advantages arising from immediate reduction and coaptation are the following: 1. The untorn portions of the joint structures are replaced at once into their normal relations; a procedure which cannot fail to influence favorably the circulation in vessels which may have escaped injury. 2. The sharp and irregular margins of the broken surfaces act as irritants to the surrounding soft tissues; immediate reduction, by placing the bones at once in mutual coaptation, acts as a preventative against the supervention of undue inflammation in and around the hip-joint. 3. With coaptation the process of repair is initiated at once, the blood and exudation material between the fragments act as a temporary cement substance, and at the same time serve a useful purpose in re-establishing the interrupted circulation. 5. Perfect reduction and coaptation prevent muscular spasms and diminish pain.

II. Having reduced the fracture, retention should be maintained in a similar manner, as in impacted fractures; with the exception, however, that eversion should be carefully corrected. The plaster-of-Paris splint is applied as for impacted fracture, only that over the trochanter major of the injured side a fenestrum, about two inches wide and four inches long, is left open for the purpose of applying lateral pressure.

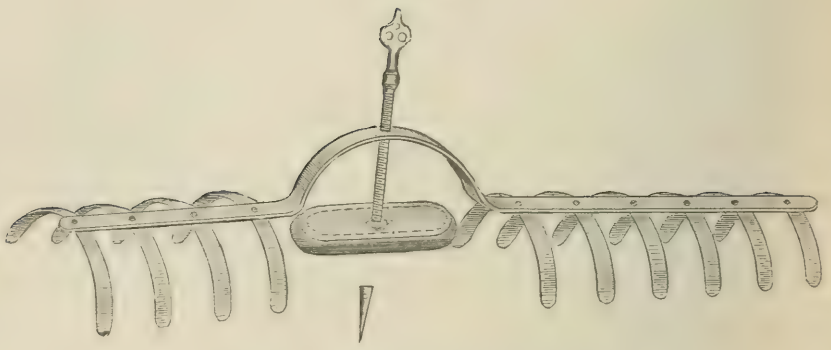
III. Many fractures of the femoral neck are kept from becoming displaced for a variable period of time by interlocking of the denticulated broken surfaces, a condition which has been called by Bigelow "rabbeting." Believing that the surgeon should imitate the reparative resources of nature wherever it is possible to do so, it appears to me that artificial rabbeting could often be produced by lateral pressure. The fractured surfaces being placed as accurately as possible opposite each other, lateral pressure would cause perfect coaptation and a mutual

interlocking of the fragments. Lateral pressure, applied with this view, would be one of the most reliable means to prevent secondary lateral and longitudinal displacements. Pressure, to be effective, must be applied in the direction of the broken neck, that is, over the trochanter major, and in such a manner as not to interfere with the superficial circulation. Pressure with belts and strips of adhesive plaster encircling the whole pelvis, can exert but little influence on the fractured bone, at the same time it impedes the superficial circulation. With the fenestrated plaster-of-Paris splint, pressure can be applied directly over the trochanter major, by placing a well-cushioned pad, with a stiff, unyielding back, corresponding in size to the fenestrum, in the opening of the splint, and applying the necessary amount of pressure by means of a Petit's tourniquet, or some other similar contrivance. A small amount of pressure, if well directed, would be sufficient to retain the fragments in apposition. By removing the pad from time to time, and washing the parts with dilute alcohol, there would be no danger of producing excoriation. The pad could also be made smaller, and the pressure surface changed as often as necessary, as an additional precaution against superficial excoriations. Lateral pressure and fixation, however, could be applied more directly and advantageously by means of a long, sharp, steel pin, regulated by a set-screw passing through the centre of a curved steel bar, incorporated in the plaster-of-Paris bandage over the fenestrum, in such a way that the sharp point of the pin would perforate the soft parts over the centre of the bone of the femoral neck, and by penetrating a small distance into the bone, would secure perfect immobility of both fragments. By removing the steel pin and adjusting the pad, this instrument can be used for applying ordinary lateral pressure. I present for your inspection an instrument of this kind. (Figs. 18, 19.)

Heine[1] has used a somewhat similar instrument in the treatment of ununited fractures of the femur and tibia. The super-

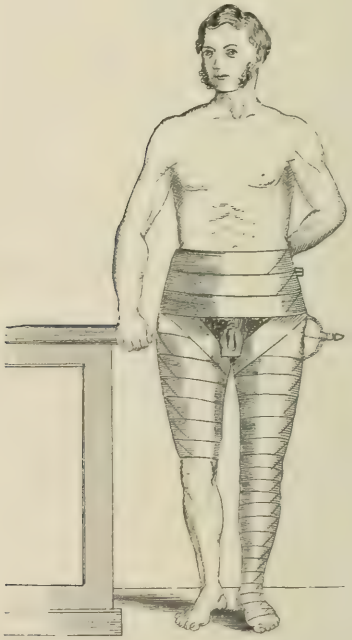
[1] Ueber operative Behandlung der Pseud-arthrosen, Verh. d. Deutsch. Gesellsch. für Chirurgie, 1877, p. 220.

Fig. 18.



Apparatus for treating Fracture of Neck of Femur.

Fig. 19.



Apparatus applied; steel point of instrument fixed in Trochanter Major.

ficial site of the greater trochanter would render this dressing of easy application, and from the absence of very sensitive struc-

tures, the pain inflicted would not be as severe as in using Malgaigne's hooks for fracture of the patella. Undue inflammation of the soft parts could readily be avoided by the use of antiseptic precautions. It would probably not be necessary to continue this treatment for more than two to four weeks, when the ordinary pad for lateral pressure could be substituted for the steel pin.

IV. Apposition of the fractured ends could be secured and maintained with the greatest degree of accuracy by measures, which are calculated to operate directly upon the fragments. Such measures have been successful on other joint fractures, where the usual prescribed methods of treatment had failed in effecting union by bone. In the case of fracture of the femoral neck, however, the injured parts are so inaccessible as to exclude the propriety of any cutting operation for the purpose of exposing the fragments to view and securing apposition through the wound. At the same time, this injury is limited almost exclusively to a class of patients whose general condition would forbid an operation of such magnitude for such a purpose. If, however, an operation could be devised which would be devoid of immediate or remote danger to life, and that would not incur any loss of blood, nor add to the suffering of the patient, and at the same time would render substantial aid in maintaining permanent apposition of the fragments, then our prospects for securing better results would indeed become more encouraging. I believe we have such an operation in subcutaneous drilling of the neck of the femur, and nailing the fragments together by means of a bone-peg.

The observations of Volkmann and Heine have shown that driving ivory-pegs into osteo-porotic bones will produce an osteo-plastic inflammation and osteo-sclerosis. The operations of drilling and insertion of bone-pegs have been resorted to for a long time, for the purpose of promoting the formation of callus in cases of ununited fractures, and it is only reasonable to assume that the same operations would have a similar effect in recent fractures. The operation offers no great technical difficulties in its execution, and if done under strict antiseptic precautions,

does not expose the patient to any additional risks. The idea of immobilizing fractures by nailing the ends of the broken bone together is not a new one. It is alluded to by David Prince,[1] in treating of the subject of ununited fractures, when he says: "Perhaps a bone might be drilled through both fragments, and held in apposition by a rivet of one of these metals. The presence of the rivet after the completion of the healing process would do no harm, and if a permanent discharge should be the result, the metal could be readily removed."

As yet a discrepancy of opinion prevails as to the future fate of bone- and ivory-pegs when imbedded in living bone. Trendelenburg[2] operated for a very oblique ununited fracture of the femur at the junction of the lower with the middle third, by fixing the fragments with an ivory-peg. He had an opportunity to examine the specimen two-and-a-half years after the operation. The fracture was firmly united, and the ivory-peg was found intact in the bone-tissue, having undergone no change whatever, except that a portion which had projected into the knee-joint had become detached, and was found imbedded in a cyst in the interior of the joint, surrounded by giant cells.

Riedinger[3] made similar observations. Introducing ivory- or bone-pegs into the bones of animals, he found them after a variable period of time either entirely unchanged, or only slightly diminished in size. The diminution in size appeared to be in proportion to the vascularity of the living bone. The growth of the bones thus treated was stimulated, as was shown from an increase in their length as compared with the opposite bones.

Bidder[4] found that by boring a hole into the spongiosa of the epiphysis of the long bones in old rabbits, into the lower end of the femur, for example, that no regeneration of bone took place, the loss of substance being replaced by fibrous and myeloid tissue. In young adult rabbits a slight attempt at regeneration

[1] Plastics and Orthopedics, 1871, p. 220. [2] Ibidem.

[3] Virchow u. Hirsch's Jahresbericht, 1881, vol. ii. p. 333.

[4] Exper. Beiträge u. Anat. Untersuchungen d. Knochengewebe, etc., Archiv f. Klin. Chir. B. XXII. p. 155.

was manifested. The process of regeneration, however, was increased by driving ivory-pegs into the perforations, or by injecting iodine or lactic acid.

Brainard[1] taught that simple perforation of bone increased the formation of callus, while insertion of ivory, wooden, or metallic pegs not only disminished it, but with few exceptions produced absorption of bone.

Volkmann[2] treated a pseud-arthrosis of the femur by excision of the fractured ends, and immobilized the fracture by driving a peg made of a piece of fresh bone, taken from another patient, into the medullary cavity of both ends. The fracture united and the transplanted piece was not seen again.

Riedenger's[3] experiments on animals have shown that ivory- and bone-pegs implanted into bone increase the nutrition of the bone, and remain without giving rise to any undue irritation, and are finally partially or completely absorbed. Metallic substances remain firmly imbedded. Wood and rubber invariably gave rise to suppurative inflammation. Clinical experience and experimental investigations have sufficiently demonstrated that bone- and ivory-pegs, if implanted under antiseptic precautions, do not act as foreign bodies, and never give rise to suppuration; and can, therefore, be safely employed in securing accurate coaptation of recent fractures, if this is deemed desirable and necessary, and not attainable by simpler measures. It has also been shown that these pegs produce osteo-plastic inflammation, and thus materially hasten the process of repair. The operation of direct immobilization of the fragments by means of bone- or ivory-pegs is, therefore, particularly adapted to the treatment of intra-capsular fractures, whenever it is decided to make every legitimate attempt to secure union by bone. A somewhat similar operation has been performed several times for the purpose of relieving the pain incident to pseud-arthrosis, following fractures of the femoral neck. Before the introduction of antiseptic

[1] Treatment of Ununited Fractures, Trans. Am. Med. Ass. 1858.

[2] Ueber Pseud-arthrosen der Vorderarm Knochen. Verh. d. Deutsch. Gesell. f. Chir., 1881, ii. p. 167.

[3] Verhandl. d. Deutschen Gesellsch. f. Chir., 1877, p. 134.

surgery, von Langenbeck[1] operated by exposing the greater trochanter, and passing a silvered drill through it into the upper fragment so as to secure apposition. The fracture was oblique and extra-capsular in an aged female. The operation was followed by destructive inflammation, hospital gangrene, and death. Lister operated in a similar manner, but under the protection of antiseptic surgery, and secured a good result by a short fibrous union.[2] In this case, however, it appears that the upper fragment was not transfixed by the screw. Koenig[3] repeated Langenbeck's operation under antiseptic precautions, and secured a favorable result. My experiments on animals have satisfied me that it is not always an easy task to find the upper fragment with the drill, and perforate it at the proper point. To overcome this difficulty, it has been suggested by Trendelenburg,[4] to expose the seat of fracture by a small incision from behind, and after forcibly abducting the limb, perforate the lower fragment from within outward, and by reinserting the drill from without inward, guided by a finger in the wound, and, after straightening the limb, transfix the upper fragment. A silver screw is inserted in the hole made by the drill, and the two fragments are screwed together. The screw is to be removed after two weeks. For the purposes for which we have urged the operation, Trendelenburg's advice is too severe and dangerous. By using bone- or ivory-pegs no disastrous result would follow in the event the peg should miss the upper fragment and be driven into the joint.

Trendelenburg's case and my experiments on animals furnish positive proof that bone- and ivory-pegs driven into the interior of joints do not give rise to any serious results. The operation of drilling the femoral neck and the subsequent insertion of the ivory-peg is facilitated by placing the limb in its natural position and securing it by the plaster-of-Paris dressing. The drilling is done through the fenestrum over the greater trochanter in the plaster splint, by sliding the skin and making a passage for the

[1] Verhandlungen d. Deutschen Gesellsch. f. Chirurgie, 1878, p. 92.
[2] MacCormac, Antiseptic Surgery, 1880, p. 200.
[3] Verhandlungen d. Deutschen Gesellsch. f. Chirurgie, 1878, p. 93.
[4] Idem.

drill through the soft tissues down to the bone with a tenotome at a point corresponding to the centre of the base of the femoral neck, and drilling in the direction of its axis toward and into the femoral head. The length of the bone- or ivory-peg should correspond to the distance between the outer surface of the greater trochanter and the centre of the femoral head. The advantages arising from the treatment as suggested above would be:—

1. The most perfect degree of coaptation and immobilization of the fragments.

2. The patient could be placed in any position in bed, or even be taken out-doors as soon as the dressing is applied, thus effectually preventing excoriations and the diseases incident to prolonged confinement to bed in the recumbent position.

I do not advise, of course, that all the means suggested should be called into use in every case. When the general condition of the patient is such as to preclude any possibility of obtaining a good result, the severer measures, which aim at accurate adjustment and immobilization, are not to be used. In such cases, it is only necessary to adjust the parts as accurately as possible, and apply the plaster-of-Paris dressing, which, by keeping the parts at rest, secures for the patient the greatest degree of comfort, and, moreover, does not exclude the possibility of recovery by the formation of a short ligamentous union. If the patient's health is fair, and the symptoms are such that point to the existence of the fracture within the capsule, then we are justified in resorting to the means which will insure the greatest accuracy in the approximation of the parts, and thus furnish nature with the only known means by which she can restore the continuity of the bone.

DISCUSSION.

The President. I have listened with profound interest to the paper just read, as every one, I am sure, must have done. This paper, when it appears in the Transactions of the Association, will give to it lasting credit. It shows a maintenance of the Association that is gratifying to me as one of its founders.

Dr. W. C. B. Fifield, of Boston, Massachusetts.

In this instrument [referring to the splint exhibited by Dr. Senn] I think I recognize an old acquaintance, long forgotten, but some of them still in existence, and which was known in my day as Malgaigne's spear. His spear was used, or attempted to be used by him, in the treatment of oblique fractures of the tibia. I saw Malgaigne himself apply it in the Hôpital Saint Louis. Malgaigne's instrument was very much like this, indeed. Erysipelas, pyæmia, and other conditions followed its use, which would probably be prevented to-day by the use of antiseptic methods; he had bad success with his instrument, and the instrument fell into the well-merited oblivion which has followed. There is one of them still among the instruments of Boston Hospital. I am in doubt whether this instrument would be applicable in the treatment of a fracture of the neck of the thigh-bone in an elderly patient. I doubt very much whether we could get a patient to submit to its application; and I doubt whether we could get better results from its use than from the ordinary treatment. It seems to me that mechanical appliances are passing out of vogue. We are putting aside splints, and we are finding that just as good or better results follow from the treatment of fractures by position. It has been shown that nothing more than the position of a part is needed in the treatment of fracture. That, in fractures at the elbow-joint, nothing but position is needed, and no ill result follows this plan of treatment. And so with oblique fractures of the tibia. Position is the best and the natural splint, and the greatest invention of any age. It has been shown, too, to be perfectly applicable to every fracture of the lower extremity. Dr. Moore has shown that, in that most dreaded fracture, Colles's fracture, no splint is needed, and that we have a better splint in the position of the limb than in anything that has been devised. And it seems to

me that if he demonstrates this to the world, he has done a good deal.

I wish to call attention to Dr. Moore's statement with reference to fracture of the neck of the femur. It does not always result from an injury. The last time I was in Paris, I saw a patient in the Hospital, who was supposed to have received a fracture from simply turning in bed, or, perhaps, in getting out of bed; at any rate, it was the result of some very slight movement. The patient died, and upon post mortem examination there could not be found a vestige of the head of the femur. The part above the point of fracture had disappeared by absorption. The history of the case was obtained, and it was found that the individual had been a long time in the Hospital with locomotor ataxia. The case was reported, and other cases were found on record. Such cases should be borne in mind, on account of the possibility of their occurrence, and of a predisposition to such fractures resulting from a condition of the bone which may exist in certain diseases of the nervous system.

The gentleman who showed, just now, a specimen of separation of the head of the bone at the epiphyseal line, suggested another thought which had been in my mind some time, and that is as to the possibility of the occurrence of an epiphyseal inflammation in injuries of the hip-joint, or in any one of the joints. That is, the cartilage being attacked by an inflammation and this extending over the bone to the point of junction, and in this way developing pain, immobility, and tenderness on pressure, with other symptoms of disease of the joint.

I think that may be the explanation in certain cases in which inflammation attacks the joint. The exception is frequent, but I am satisfied that it occurs much more frequently than any of us would be willing to admit.

Another thing may be remembered; and that is, that, where the epiphyseal cartilage is injured, growth may be arrested. So that with an injury of an epiphyseal cartilage, the growth of the limb may be arrested, and shortening take place.

Dr. C. B. Nancrede, of Philadelphia, Pennsylvania.

I wish to ask Dr. Senn if I understood him correctly, when he said that the specimen in which the bony union had taken place was one of intra-capsular fracture. I would like to call his attention to the variable attachment of the posterior segment of the

capsular ligament. I have often seen this portion attached nearly two-thirds of an inch from the posterior inter-trochanteric line. I suppose his case to be one in which the attachment was nearer to the femoral head than usual, and in consequence part of the fracture was really *extra-capsular*.

The PRESIDENT. It strikes me that the specimen of Dr. Senn could never have been a fracture, unless the surgeon who treated the case was a surgeon possessed of such skill as we never see in our age. The ends of the alleged fragments are in such close contact that it precludes the possibility of its ever having been a fracture.

Dr. MOSES GUNN, of Chicago, Illinois.

I did not intend to speak upon this subject, but since it has been introduced, I simply wish to relate a circumstance which occurred last year with reference to this very specimen. Dr. Senn submitted this specimen for examination after his departure, as a sample of bony union after fracture within the capsule. On examination, I was able to distinctly trace the capsule, which was then preserved, past the line which was taken for the line of fracture, so that there appeared an unmistakable connection with the outer line of fracture. The outer extremity of the capsule crossed behind this, and the capsular ligament had an attachment to the outer end of the internal fragment. I remarked at that time that it was not a case of fracture within the capsule—that is, surgically speaking—for I think we must make a distinction between the surgical and the anatomical construction of the position of this ligament. It is only a few weeks since I was demonstrating the anatomy of the hip-joint, and I was much surprised to find on the back side of the joint a frænum-like capsule, vascular, thick, and firm, which ran clear up to the head, and which, if there had been a fracture clear up two-thirds of the way to the head, would have allowed the upper fragment a good supply of blood. The specimen of Dr. Senn has since been boiled, and does not now show the capsule. I remember stating at that time that I did not believe it was an intra-capsular fracture with bony union. I have never seen a bony union after intra-capsular fracture.

Dr. R. A. KINLOCH, of Charleston, South Carolina.

I am sure that every Fellow of the Society must, like myself, feel

greatly indebted to both the gentlemen whose papers have just been read, and are now under discussion. I must confess that, after hearing the papers, I felt envious of the great ability for work that their authors have exhibited.

It has been said that comparisons are odious, and yet I cannot fail to contrast the character of the work done by our two friends. The one has offered us mainly the result of *practical work*, accomplished by ordinary and simple means well known to the profession, and by the exhibit of the specimens upon the table, has shown us how well his work has been accomplished. The other has presented us a deeply interesting volume, that only could have come of prodigious labor and intense interest in the subject treated. The papers illustrate, from different standpoints, the therapeutics of one surgical subject. In the one we have offered results, good in kind, coming from the execution of carefully planned, common-sense surgical methods. The practice illustrates the pathology. In the other, we have ingenious experiment which would develop a pathology upon which to determine practice. I trust that no one of us will take offence at a criticism of views, and I must confess that the latter paper is too full of pathology for me. Dependence upon so much pathology would frighten me from the practice of surgery. I do not desire to decry pathology, or to say that it does never found true principles of practice. But I regard it as not so fixed in this case as to permit of our leaning on it.

I am a believer in the possibility of bony union in intra-capsular fractures; but my faith does not come of such experiments, and such ingenious theories as have been advanced. I take it, we are here as practical surgeons. We all claim to speak somewhat from actual experience with the living man, and we would rather know and hear of *facts observed* than to listen to pathological doctrines. The experiments on animals are but of little value unless the author can tell us how these have established correct treatment in similar lesions pertaining to man. I was glad to hear one of the Fellows ask the learned author of the last paper if he had treated any cases by his improved method, as founded upon his experimental pathology. The question has not been answered. I would go further, and ask if the operative procedures, the surgical manipulation described, has ever been demonstrated on the cadaver? Have the ivory-pegs ever been applied so as to fasten the fragments of the neck together, when fractures have been made upon the dead sub-

ject? No one disputes the advantage of such procedure in cases of fractures of bones that can be easily reached, as the tibia, patella, etc. I take it, the fragments of the neck of the thigh-bone cannot be so easily manipulated. Given any fracture of this kind, who can tell the precise direction of the line of fracture? Who can say if the fragments can be put in such apposition, as to be connected with ivory-pegs? The manipulation of the bone of a cat proves nothing as to the feasibility of a similar operative procedure on the living man. And when our friend comes here and advises a method for securing apposition of fragments, and consequent osseous union in a human subject, he must tell us how often he has accomplished this feat. I don't expect to see the surgeon who will do this operation successfully.

The apparatus before us, proposed for lateral pressure, modelled after the instrument of Malgaigne, which has often been successfully applied to the tibia, I think may be occasionally of service, but I hardly think it necessary. If it is used, I should not fear, as some do, that erysipelas or pyæmia would follow. I have never seen such results in the few cases where I have used the contrivance in fractures of the tibia.

But, to conclude, Mr. President, I thank my friend again for all he has done. I have been instructed and have listened with pleasure to much that he has said. Yet he must excuse my want of faith in his proposed surgical practice. I want to see his operations demonstrated, and I want to see the results of his practice before I can become a convert to his views.

Dr. H. F. Campbell, of Augusta, Georgia.

I agree with Prof. Kinloch as to the impracticability of applying this apparatus. Suppose we could cut down upon the fragments of the fractured bone and pin them together, and hold them there, would they unite? We must consider what is usually the condition of the patient; what is the material we are working with, and what was the cause of fracture. The nutrition of the bone was at fault. It was a retrograde metamorphosis in that part of the bone, at the expense of all the structures of the bone. It breaks because it has become brittle from senile influences affecting its nutrition. This portion of the femur, at best, is badly supplied with blood. I do not think that if the fragments were held together they would unite. However much we may admire the investigations, or however proud

we may be that there is among us a Fellow whom we may envy for his talent and perseverance in pursuing these investigations, we should hesitate before laying aside the simple treatment we have so long employed, and which suffices to give the power of locomotion to the patients who have suffered from fractures of the neck of the femur. While we may not be able to obtain the best results from this treatment, we should never endorse a treatment that is indicated only by experiments.

Dr. SENN. I will be brief. I treated the case reported at the beginning, and consequently have the benefit of having made the examination soon after the injury occurred. The patient was a woman, seventy four years of age, who slipped on the ice and fell. Two days after she was admitted to the Hospital, we found shortening of the extremity to the extent of half an inch. There was no asymmetry of the thigh-bones. Consequently I attributed the shortening to the fracture of the bone. The foot was strongly everted. With this shortening, the absolute loss of function, the age of the patient, and the manner in which this injury was received, I assumed that the individual was suffering from an impacted fracture of the neck of the femur. The patient was not treated with this instrument; she was simply treated on the expectant plan—rest in bed in the recumbent posture, continued for three months. She suffered the characteristic pain in the region of the trochanter minor. At the end of three months she was allowed to walk about on crutches. But I found that the shortening increased after the second or third week; shortening due to inflammatory osteo-porosis and interstitial absorption. I had an opportunity two years after to make a post-mortem examination, the patient having been re-admitted and dying of pneumonia; so that I have the clinical history of the case complete after the fracture. When we again measured the femur, we found that the shortening had increased from one-half of an inch immediately after the injury to one inch and a half. You can see even now, in the specimen, evidences of impaction, in the shape of a dense plate of bone in the interior of the femoral head, indicating the apex of Adams's arch. Indeed, I think the result demonstrates in itself sufficiently that the fracture was an impacted one. I find now in the specimen, that the head of the bone has undergone the peculiar change of position which we find generally in cases of fracture of the neck—it has descended to a level with

the upper margin of the trochanter major and is directed backward. I find along the anterior portion of the neck a distinct line of fracture. Where the two fragments meet you notice a denticulated surface near the centre of the anterior surface of the neck; but that an impaction has occurred along the posterior wall. On the posterior surface it is easy to trace the impaction of the posterior wall into the head. I find in the interior a compact structure, which can be nothing else than the impacted portion of Adams's arch. These conditions, I claim, are sufficient to prove the existence of an impacted fracture within the capsule. To prove this, we must have, first, the history of the existence of a fracture during life; again, there must be no evidence of a fracture without the capsular ligament. All these things can be readily demonstrated in the specimen.

So far as this instrument is concerned, I expected to meet with opposition. I do not claim originality for it. All that I claim for this instrument as being superior to Malgaigne's is, that as it is to be incorporated in the plaster splint, it adds additional strength and serves at the same time the useful purpose of a splint. No one can object to making lateral pressure in these cases.

It is said that the want of lateral pressure is an important element in preventing bony union. So that in pressing the fragments together, we not only bring the ends together, but we add an element for the union by the pressure. Here, then, is the greatest element of danger in intra-capsular fractures. I have shown illustrations of three specimens, described by gentlemen whose opinions admit of no discussion.

The *greatest danger* that these patients are exposed to *is the old treatment!* You must all admit that the worst punishment you can inflict upon an old patient is the recumbent position in bed continued for any length of time. What is more common in these cases than that the individual becomes the subject of bronchitis, bronchopneumonia, or pneumonia, which almost invariably prove fatal in a short time; so that the old gentle treatment has, only too often, been the direct cause of the great mortality attending this class of injuries. It adds to all the trouble the danger of complications. Under the treatment suggested, your patient can be removed from bed and receive the benefits of open air, at the same time accurate coaptation and retentiou are maintained.

So far as local irritation is concerned, it needs no discussion. You will not claim that the small opening that is necessary to intro-

duce this steel point into the bone will become the seat of septic infection. I have not used this instrument, but I shall use it; because with the old treatment I have not obtained the benefit that has resulted from my experiments on animals. These animals were generally young animals. I have not experimented on the cadaver. My advantages in that direction are quite limited. But any person who can hit the head of the femur in a cat will certainly not miss it in operating on the human subject. As to resorting to this operation, it would be no great detriment to your patient if you did go amiss of the bone, provided the operation is done antiseptically.

Dr. GUNN. Did not Dr. Senn have a case in which the peg was not absorbed?

Dr. SENN. No; I had a case in which a portion of the peg was found encysted and surrounded by giant-cells.

Dr. NANCREDE. Dr. Senn answered our question negatively, not having used the apparatus. He says it would be proper to use it antiseptically. Now it would be impossible to use it antiseptically. If he attempts to put on the antiseptic dressings with this instrument over them, he violates the rules of the method.

Dr. SENN. It is not necessary to apply the Lister method. I simply said "with antiseptic precautions." You can do that very well by supplying its place with iodoformized earth or salicylated cotton dressings. By carefully packing the margins of the splint with the earth, or packing the cotton around it, you can readily prevent the access of air, and thus effectually guard against septic infection.

Dr. E. M. MOORE, of Rochester, New York.

I wish, before saying anything, to render my thanks very heartily to Dr. Senn for his elaborate experiments.

I have this feeling about bony union in intra-capsular fractures. I suppose it must be admitted that the rule for intra-capsular fractures is that repair must occur by ligamentous union. But when we state as a fact that there are no exceptions to this rule, we take ground which it seems to me is untenable. Now, it seems to me, it would be a strange thing if in this matter we should find no single exception. The object of my paper was not to deny that the exception might occur.

Now I would like, in a few words, to put the pith of my paper before your minds. It was simply this: I showed you three specimens of fracture of the neck of the femur which were produced by a fall upon the trochanter, and in which death occurred three, five, and eight days after receipt of the injury. We found them all with the cervical ligament torn only partially, and the limb markedly rotated. We found them all with the necks shortened to a marked degree. What is interstitial absorption? In my paper I speak of the fact that absorption does occur. Nature leaves no rough surfaces, she always rounds them off. We have all seen that union occurs by means of ligament, and absorption occurs, and we may have all the bone removed. There is no question about absorption; there is plenty of it under such circumstances. I find here what I did not expect to see. Here is a specimen in which there can be no question about bony union; it is almost ivory; and here I hold up before you a specimen in which only three days elapsed after the injury. How much absorption is necessary to make this one as short as the other? You have this crowded right up close. After union occurs we have no need of absorption.

Now another point that I made. It was this, that if you had absorption occur as the result of inflammation, the inflammation would extend through the whole bone-tissue. We have the parts then pressed down so as to alter the shape of the neck. Where then we have union taking place with the alteration of the head, it would be unfair to say that it was the result of absorption and of inflammation. I have compared these two specimens to show the great change that has occurred with reference to the size. If you look in Dr. Robert Smith's book, you will find these figured in a great many femurs as cases of rheumatic arthritis. Now here was a man who fell upon his hip, and as he fell upon his hip, it was natural to suppose that he sustained an intra-capsular fracture, and the specimen we have shows the fracture.

One word now as regards Dr. Senn's observation. I do not see how he can rely on anything in this case that shows there was impaction. I regard these three as all remarkable specimens. I believe they represent the condition we have to deal with all the time. Now, here we have impacted fracture—that is plain; we can all understand that. Here is another specimen in which the patient died after four years, in which there is impaction also. Dr. Bigelow speaks of a case in which the spiculæ were driven in up to the head.

I want to criticize Dr. Senn's treatment. He says he would pull the limb down and adjust the fragments. I believe that would be an error. What do you want to pull it down for? If you pull it down so as to get the proper length of limb, you overcome the shortening of half an inch or so, and you break off these spiculæ. You must set the shortened broken surfaces against each other without expecting to get the right length of the limb. The right way is to take it as it is. Then we know the cervical ligament is drawn out an inch or two. It is because the bone is crushed upon the posterior part of the neck that eversion occurs. Your patient's sensations will be the guide as to the weight which should be employed. I usually apply the weight with a basket. I put some weights in the basket and then test it, taking the patient's sensation as the guide. In that way I have gotten greater satisfaction than by the use of any splint I have tried.

Dr. Senn. I am laboring under the disadvantage of having read only one-sixth of my paper. Under the subject of impaction I have explained what Bigelow has called crushing in of the posterior wall. Crushing cannot occur without some degree of impaction. The spongiosa is crushed to an extent corresponding to the depth of impaction. The posterior wall is driven into the head a certain distance with more or less crushing of the spongiosa. The symptoms I have described are to me quite diagnostic during life—a moderate amount of *shortening* and *eversion*. I do not attempt to elicit crepitus, because I consider that the manipulations necessary to elicit this symptom, result in more harm to the patient than benefit to the surgeon. So that in rendering a diagnosis we rely on the two most important symptoms, *shortening* and *eversion*.

Dr. Moore. I do not think Dr. Senn has understood my question. What I understand by impaction is, that the spiculæ are driven into the head.

Dr. Senn. That is what I also understand.

Dr. Moore. Now, in the specimen presented by you, there is nothing of this kind, and my supposition is, that, as bony union exists, impaction was not present.

The President. Eversion of the limb and shortening are two most fallacious symptoms of fracture within the capsular ligament.

Eversion may occur from any injury to the trochanter major. It is the natural position of the limb when we lie, and, of course, it falls into this position when any injury has occurred. As for shortening of one-half an inch, it is nothing more than is often found in the natural state. I consider the specimen of Dr. Senn as nothing more than an example of senile change; and I do not see any similarity between the fracture presented by Dr. Moore and that presented by Dr. Senn.

A PECULIAR FORM OF TRAUMATIC ANEURISM OF THE FEMORAL ARTERY.

With Exhibition of the Specimen.

By T. G. RICHARDSON, M.D.,
PROFESSOR OF SURGERY IN THE MEDICAL DEPARTMENT OF THE UNIVERSITY OF LOUISIANA, NEW ORLEANS.

[Read June 2, 1883.]

The specimen which I exhibit is of such an unusual character that I desire to submit it for examination to the Fellows of the Association with the following history of the case:—

Louis Bickham, born in 1834, received a pistol shot in the left thigh, October, 1876. The ball entered in front and a little upon the inner side of the axis of the limb, about four inches above the patella, ranged slightly downward immediately across the track of the femoral artery, and emerged in the upper angle of the popliteal space just external to the median line. The hemorrhage was slight and the wounds healed soundly in a few weeks, no lameness or other discomfort remaining.

In April, 1878, about eighteen months after the accident, the patient discovered a small, soft, deep-seated tumor on a line between the anterior and posterior cicatrices, but as it was neither painful nor otherwise inconvenient, no special attention was given to it. However, it continued to grow, and in fourteen months had attained the size of a goose's egg; but even then it was not the source of any great discomfort. But, about this time, the limb below the tumor became suddenly one day numb, swollen, and congested, and did not recover its normal state until after elevation, frictions, and heat had been resorted to and maintained for twenty-four hours. Shortly after this, in May, 1879, he was persuaded to consult his family physician, Dr. C. J. Bickham, who at once recognized a traumatic aneurism, and placed him under my care. Upon exami-

nation I found a large, irregularly fusiform, pulsating, fluid tumor, extending from the middle of the thigh along the course of the femoral artery into the upper angle of the popliteal space. It measured seven or eight inches in length and four in breadth, and increased the circumference of the limb three inches, as compared with the opposite. General expansion synchronous with the arterial pulse, and aneurismal bruit were remarkably distinct, owing mainly to the leanness of the thigh and the comparative thinness of the walls of the sac; but it was observed that when the artery was compressed in the femoral triangle that, although all pulsation ceased in the tumor, *the size of the latter could not be diminished to any considerable extent by grasping it firmly in the hand.*

The diagnosis already made by Dr. Bickham, was unhesitatingly confirmed, and, taking all things into consideration, I deemed it a particularly favorable case for treatment with the elastic bandage. Accordingly, a few days afterwards (June 14, 1879), the patient having taken a bed temporarily at the Charity Hospital, with the assistance of Dr. B. and other professional friends, and the resident students of the Hospital, this procedure was fairly instituted. After thirteen hours' trial, intermitting the bandage from time to time, and substituting digital compression or the tourniquet, no decided coagulation could be detected, and the patient having become pretty thoroughly exhausted, the treatment was suspended. The following record was made at the time of the application of the bandage.

June 14, 1879. 10 A. M. Applied Esmarch's bandage from foot up to and beyond the tumor, leaving the latter uncovered. Did not use the tubing, as the circulation was completely controlled without it. Gave one-third grain morph sulph. hypodermically.

10.30 A. M. Began inhalation of sulphuric ether to subdue pain, and continued it in a mild way throughout the operation.

11.30 A. M. Suffering necessitated removal of bandage after a May's tourniquet had been adjusted to the artery in the lower angle of femoral triangle.

3.50 P. M. Reapplied elastic bandage, covering the tumor lightly, and substituting the elastic tubing for the tourniquet.

4.30 P. M. Reapplied tourniquet and removed bandage. Tumor seemed to be partially solidified, but upon slightly relaxing the tourniquet decided pulsation was felt.

7 P. M. Tumor feels somewhat less fluid.

8 P. M. Relaxed tourniquet and pulsation immediately returned.

9 P. M. Removed the tourniquet and applied bag of shot weighing five pounds, which was sufficient to control the circulation. Stopped the anæsthetic and gave morph. sulph. one-third grain, hypodermically.

11 P. M. Pulsation quite strong despite the weight upon the artery. Patient greatly distressed. Treatment arrested.

I then resolved to ligate the artery so soon as the patient's general condition would permit. July 10th, everything being favorable, the operation was performed under the carbolic spray. I employed a carbolized animal ligature which unfortunately broke. Without attempting to remove the loop, I passed another ligature alongside, and having tightened it satisfactorily, cut off the ends of both, closed the wound with sutures of the same material, and applied linen compresses and roller bandage wet with carbolized water. The dressings were kept constantly moist with the antiseptic, and no sign of undue inflammation showing itself, they were not disturbed for ten days, when, upon their removal, the wound was found entirely healed.

The second day after the operation the tumor was found to have enlarged, the limb at this point measuring at least an inch more in circumference than before, and the contents as fluid as ever. From this time the tumor very slowly contracted, and on the sixth day I thought I discovered evidences of coagulation taking place, which was subsequently proved to be a mistake. At the end of two months the tumor was only a trifle smaller than it was prior to the operation; its contents were still fluid, but not the faintest bruit or pulsation could be detected.

The patient now resumed his usual occupation, and I saw him only at long intervals. The following April, more than nine months after the application of the ligature, I showed him to Professor S. D. Gross, then a guest at my house. No material change had occurred. The learned professor examined the tumor carefully, asked many questions, made few comments, and shrugged his shoulders in a way which evidently meant that he doubted the accuracy of the diagnosis.

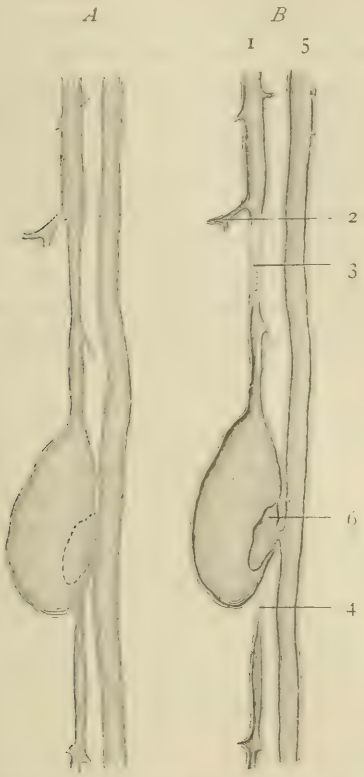
Another year elapsed, and still the tumor exhibited no appre-

ciable change in size, shape, or consistency, and there was not the faintest evidence of pulsation or bruit. I proposed to draw the fluid off with the aspirator, and endeavor to close the cavity by compression, or by some stimulating injection, but as the swelling was a matter of little or no consequence to the patient, he objected to any instrumental interference. However, a few months later, in December, 1881, a serious change occurred. At this time I was called to see him in consultation with Dr. Bickham, and found all the veins of the limb below the knee in a state of thrombosis, the foot and ankle cold and insensible, and the skin discolored and mottled as high as the middle of the thigh. There was, however, no pain, tension, or sensitiveness in the tumor or its immediate vicinity. In a day or two more the foot and lower part of the leg having begun to mummify, amputation of the thigh was determined upon. The circular operation was performed December 28th, a little above the middle, and in my attempt to save as much of the limb as possible the amputating knife passed through the apex of the tumor, where the latter was continuous with the artery. A gush of venous-looking blood was the result, and for an instant the parts were so deluged by the flow as to interrupt the operation. When the limb was severed the expanded end of the femoral artery which formed the apex of the sac was trimmed off with scissors and tied. Ligatures were also placed upon several other arteries, which were all more or less enlarged. The stump did badly from the beginning, and in less than forty-eight hours the flaps were gangrenous. This was doubtless due to the patient's enfeebled condition, the result, in part at least, of the habitual use of morphia in large quantities, for several years previously. Death occurred on the fourteenth day.

Dissection of the amputated limb, and, subsequently, of the stump, disclosed the following conditions:—

The femoral artery, at the seat of ligature, was converted into a solid fibrous cord up to the origin of the profunda. There was dilatation of the profunda and its perforating branches, but not as great as I anticipated. The arteries higher up did not appear to

have undergone any change. Immediately below the obliterated segment of the femoral, the lumen of the vessel was well preserved, and just at this point an anomalous branch[1] of considerable size was given off. The distribution and anastomosis of the latter could not be determined in consequence of the gangrene. Beginning at a point about four inches below, the main trunk gradually dilated to



A. Femoral artery and vein with aneurismal sac unopened. *B*. Interior view of same. 1. Femoral artery. 2. Anomalous branch. 3. Obliteration by ligature. 4 Popliteal artery obliterated. 5. Femoral vein. 6. Pouch communicating with vein.

form the walls of the aneurism. The latter, oval in outline, except at one point where it was slightly pouched, measured seven inches in length and three and a half inches in breadth at its widest point.

[1] This was not the anastomotica unless of anomalous origin.

Its walls were dense, and about one-fourth of an inch in thickness throughout nearly their entire extent. Its interior was flocculent or scurfy, with here and there a few filaments of fibrin attached. Upon its posterior wall, looked at from within, and a little below the middle, was seen a remarkable knobby protruberance, an inch and a half in length, an inch in thickness, projecting into the cavity of the sac. This was found distended with coagulum. Although most diligent search was made, *no communication with the sac could be found, except the entrance to the main artery above.*

The artery upon the distal side of the tumor was continuous, with the exterior of the latter opposite the nodular protuberance just described, but was obliterated, and at the point of junction spread out in a membranous form. Below this point, to the extent of an inch, it was represented by a small, flattened, fibrous cord, which gradually enlarged and became again pervious as it traversed the lower half of the popliteal space, but was not more than one-half the natural size. It was here filled with a coagulum which extended to the remote subdivisions of the anterior and posterior tibials.

The femoral vein was smaller than usual, and its walls much thickened. It was closely connected with the tumor, indeed, absolutely continuous with it, along the base of the protuberance ; and, upon laying it open, it was found to *communicate with the interior of the latter* by a smooth, well-defined orifice, three-fourths of an inch in its longest diameter. It was occupied throughout by a firm clot.

Report of Committee on Specimen submitted by Dr. Richardson.

Your committee would respectfully report that, after as thorough an examination of the specimen as circumstances admitted of, they have arrived at the following conclusions :—

1. That the original injury consisted of a contusion of the walls of both the artery and vein with consequent adhesion at the site of injury.

2. That the inflammatory processes culminating in the adhesions of the two vessels resulted in such a weakening of the arterial walls,

probably coupled with a limited destruction of the external coats of the vessel by the vulnerating body, as to render an aneurismal dilatation a necessary consequence.

3. That the similar probable partial destruction of, and certainly the inflammatory softening of, the venous walls at the point of adhesion, resulted in a slight pouching towards the lumen of the artery.

4. That the projection thus commenced, however minute, was acted upon to the utmost advantage by the arterial current, which tended to enlarge the growth by an actual traction force, as it were, in the direction of blood-flow; this is proven by the fact that the small sac is elongated on the distal side of the venous opening.

5. That the projecting venous sac must have served to obstruct the lumen of the artery from the moment of its formation, and have acted somewhat in the way an embolus has been known to do, as the exciting cause of aneurism by producing undue intra-vascular pressure on an artery with softened walls.

6. That the obliteration of the lower extremity of the artery, which occurred subsequent to the ligation, while an anastomotic branch communicated through the patulous lumen of the artery with the sac, is the probable cause of the non-coagulation of the sac-contents. The apparent paradox that blood can enter into and emerge from a tensely filled sac, is rendered clear when the physical surroundings of the sac are considered. The muscles of the region must have alternately partially emptied the sac by their compression, and allowed it to refill when they relaxed. A familiar demonstration of this is the action of the ordinary rubber-bulbed pipette, although the elasticity of an aneurismal sac is not nearly so great. This deficiency was, however, made up by the constant pressure of the blood in the patulous upper portion of the vessel, which tended to rush into the aneurism whenever the relaxation of muscle pressure upon the sac permitted.

Your committee submit the above with some diffidence, in view of the obscurities surrounding any investigation as to the etiology of this case. They feel, however, that while they have not perhaps explained all the facts of the case, their hypotheses more nearly approach a satisfactory solution of the questions involved than any others which have suggested themselves.

CHAS. B. NANCREDE, *Chairman*,
R. A. KINLOCH.

DISCUSSION.

Dr. B. A. Watson, of Jersey City, New Jersey.

I wish to make a few remarks with regard to the treatment which was adopted in this case. Dr. Richardson informs us that "after the incision was made, the artery was ligated with the catgut ligature." The interesting point is that having ligated the artery with a catgut ligature, they closed the incision with the same kind of suture. Then the wound was covered with linen compresses wet with carbolized water. No examination of the wound was made for ten days from this time. He then goes on to state that there was no discharge; that he obtained union by first intention, which is a matter of great importance. On the tenth day the dressings were removed and the parts examined. It appears that the wound was perfectly closed. There was no sign anywhere of inflammation; and there had never been any inflammation. The result was so good because there had been no cause for inflammation. He removed the dressing in the same order as he put it on, on the first day, and what was the result? It was all that it could be. His treatment was strictly aseptic. There was no irritation. Although the carbolic acid came in contact with the wound, there was no absorption. The treatment was as good as the Lister treatment. It is particularly interesting, when we compare it with the report of another case. I refer to the case reported by Dr. Packard, of Philadelphia. He amputates after ligating the femoral artery. His amputation was a re-amputation, because of septic infection. Now when he ligated the artery, I infer, although he did not state it, that he used the silk ligature. He said that when he made his secondary ligation, a sound passed clear down along the track of the ligature to the artery.

After a lapse of six days it is found necessary to ligate the common iliac. He uses again a silk ligature, and suppuration occurs.

Now the preliminary operation was one of the best illustrations of the result that can be obtained from this method of practice. Make that treatment strictly Listerian, and suppuration will not occur.

A CASE OF NEPHRECTOMY FOR MEDULLARY CARCINOMA, AND PARTIAL CHOLEO-CYSTECTOMY FOR CALCULUS, IN THE SAME SUBJECT.

By SAMUEL W. GROSS, A.M., M.D.,
PROFESSOR OF THE PRINCIPLES OF SURGERY AND CLINICAL SURGERY IN THE JEFFERSON MEDICAL COLLEGE OF PHILADELPHIA.

[Read June 1, 1883.]

On the 14th of April, 1883, I was requested by Dr. William V. Keating to see with him a widow, fifty-nine years of age, who first noticed a small and painless tumor in the right iliac fossa, three months previously. The tumor occupied the lower right lumbar and inguinal regions and about one-third of the hypogastric region, was apparently as large as a child's head, nodulated, of tolerably firm but unequal consistence, mobile, everywhere dull on percussion, there being not the slightest evidence of the presence of the intestines in front of it, and a few drops of bloody fluid were obtained with the exploring needle.

Dr. Keating informed me that the patient had an attack of hæmaturia towards the end of February, which was repeated twice in March, the last requiring the internal administration of ergot for its control. About the middle of February, she began to have pain in the loins, and especially in the right one, which increased in severity with the growth of the tumor. From an examination made two weeks before I saw her, Dr. Keating excluded disease of the uterus or ovary, and diagnosticated carcinoma of the kidney. With the exception of a somewhat sallow and worn appearance, and attacks of migraine, the patient was in her usual good health.

The existence of a rapidly growing tumor and hæmaturia

certainly pointed to malignant disease of the kidney; but the mobility of the mass, the fact that it was first observed in the anterior and lower portion of the belly, and that a lateral origin could not be traced, along with the absence of some portion of the intestines in front of it, rather favored the idea of sarcoma or carcinoma of the ovary. In his recent work on *Ovarian and Uterine Tumors*, Mr. Wells states that neoplasms of the kidney are fixed, that they are situated behind the intestines, and that they are originally detected between the false ribs and the ilium, whence they extend first toward the umbilicus, next into the hypogastrium, and lastly downward toward the groin. As these symptoms were reversed in the case under consideration, it is quite evident that, however reliable they may be when the growth is small, they cannot be depended upon when the growth is large. The diagnosis was also obscured by the condition of the bladder, the fundus of which was found to be tender on exploration with the sound, so that it might have been a possible source of the hemorrhage, particularly as the urine contained no other foreign elements than blood-corpuscles.

Under these circumstances, it was determined to advise the patient to submit to an exploratory incision, with the understanding that the question of further interference be left to our judgment.

With the assistance of Dr. Keating, Dr. Mears, Dr. Hearn, Dr. Wirgman, and Dr. Newman, the operation was performed, under ether, and with strict antiseptic precautions, on the 20th of April. An incision was made from two inches below the umbilicus along the linea alba for three inches, and hemorrhage arrested with artery clamps, before the peritoneal cavity was opened. Exploration of the tumor showing that the kidney was the organ involved, the incision was prolonged to within two inches of the tip of the ensiform cartilage, when the enormously enlarged and fatty right lobe of the liver was seen to extend into the upper right lumbar and umbilical regions, the gall-bladder, distended with bile, and occupied by a stone as large as a Spanish olive, reaching almost to the umbilicus. The peritoneum covering the anterior surface of the tumor was pervaded by large vessels. This was divided to a slight

extent, and rapidly peeled off with the fingers, the procedure being attended with considerable venous hemorrhage. On account of the friability of the tumor, and the projection of a fungous mass along the side of the pelvis of the kidney, it was thought best not to incur the risks of hemorrhage by endeavoring to ligate the ureter and renal vessels separately. Hence the pedicle was transfixed, and tied in two portions with stout carbolized pure silk threads, and the tumor severed from its attachments. Furious bleeding occurred from the renal artery; but Dr. Mears succeeded in grasping the pedicle with his fingers, when I was enabled to encircle it with a catgut ligature, through which further loss of blood was effectually prevented. At this stage of the procedure, the color had left the face, and the pulse was very feeble. The calculus was then drawn down into the fundus of the gall-bladder, both of which were cut away after the latter had been secured with a catgut ligature. The peritoneal cavity having been carefully cleansed of blood, the edges of the incision were united with silver sutures carried through the entire thickness of the abdominal wall; a drainage-tube was inserted in the lower angle of the wound, and carbolized gauze was secured in position with a flannel bandage. The operation occupied fifty-five minutes, and, towards its completion, brandy and iced water were freely administered by the mouth, with the effect of steadying the pulse and relieving the pallor.

The patient reacted well, and at 4 P. M., or three hours after the operation, a hypodermic injection of morphia and atropia was administered on account of suffering. At 8 P. M. the pulse was 108, and the temperature, taken by the mouth, was 100.1°. The catheter drew off three ounces of urine.

April 21. The patient passed a comfortable night, until 4.30 A. M., when she complained of slight nausea and pain. Brandy and iced milk have been administered every three hours. The urine was drawn off at 6 A. M. Morning temperature 99.4°, and pulse 100. The bladder was evacuated at 9.30 A. M., and again at 2.30 P. M., and a subcutaneous injection of morphia given at the latter hour, which was repeated in three hours. Evening temperature 100.1°; pulse 104. Urine drawn off at 8.30 P. M.

22d. At 11 P. M. last night the patient was seized with a slight chill, and vomited a small quantity of bile. The temperature was 98.4°, and the pulse 130, which had risen respectively to 103° and

148 at 12.30 A. M. At 1 A. M. half an ounce of urine was evacuated; an hour later, she had quiet delirium; and at 3 A. M. she had another hypodermic injection of morphia to relieve her great pain and restlessness. A 6 A. M. the catheter failed to bring away any fluid, and from this time until her death, twenty-three hours later, there was complete suppression of urine, for which infusion of digitalis and hot applications to the left lumbar region were ordered. At 8 A. M. the pulse was 132, and the temperature 102°. On dressing the wound, the parts were found to be perfectly dry, and as there had been no escape of fluid through the drainage-tube, the latter was removed. There was some tenderness in the right loin and slight tympanites in the left lumbar region. Dressings reapplied. 11.30 A. M., pulse 138, and feeble. 6 P. M., pulse the same; temperature 97°; hands cold; intellect clear; voice strong; tongue moist. Morphia administered by the skin. 10 P. M., pulse flickering and scarcely perceptible.

23d. At 1 A. M. patient became unconscious, and was very restless, and death ensued at 5 A. M., or sixty-five hours after the operation.

On post-mortem inspection, the wound was found to have united throughout the greater portion of its extent. The peritoneal cavity contained about six ounces of blood-stained serum, and some of the coils of the small intestines were livid, and slightly adherent to one another. The left kidney was apparently sound and slightly congested. The bladder was empty and contracted; the liver was greatly enlarged and fatty. The stumps of the pedicle of the kidney and of the gall-bladder were in an excellent condition. The viscera and lymphatic glands were free from secondary growths.

The kidney weighed eighteen ounces, and measured fifteen inches and a half in its long circumference, and ten inches and a quarter in its short circumference. With the exception of one inch of its upper extremity, it was converted into a medullary tumor which, on subsequent minute examination, proved to be carcinomatous.

The fatal issue was evidently due to peritonitis, complicated by suppression of urine, which was not secreted for twenty-eight hours before death. Indeed, the total amount drawn off during the first thirty-seven hours scarcely amounted to sixteen ounces; and it was for this reason that we were somewhat chary in administering opium. In the way of nourishment, she took milk, brandy, and champagne, all of which were well borne by the stomach.

The kidney has been extirpated for sarcoma and carcinoma twenty-two times, the operators having been Czerny in five cases, Kocher in two cases, and Wolcott, Martin, Langenbuch, Jessop, Hueter, Lossen, Barker, Byford, Bardenheuer, Whitehead, Hicquet, Luecke, Adams, Thornton, and myself, respectively, in one case. The nature of the tumor is recorded as sarcoma of the kidney in 14; carcinoma of the kidney in 3; encephaloid of the kidney in 3; sarcoma of the capsule of the kidney in 1; and perinephritic sarcoma in 1. Of the 22 cases, 1 was under treatment at the date of the report, 9 recovered, and 12, or 57.14 per cent., died. Fifteen of the operations were by the abdominal incision, of which 5 recovered, and 10 died; and 7 were examples of the lumbar incision, of which 4 recovered, 2 died, and 1 was still under treatment. The causes of death were primary hemorrhage in 2; collapse ten hours after furious bleeding in 1; peritonitis in 3; septic peritonitis in 1; and shock, pulmonary embolism, uræmia, septicæmia, and exhaustion from profuse purulent discharge each in 1.

It will thus be seen that, apart from the risks common to all ventral and lumbar incisions, the great danger is from hemorrhage, which, even if it does not terminate fatally, may be expected to be considerable in the majority of cases, from the renal artery, the renal vein, particularly on the right side, or from one of the large veins ramifying over the tumor. Hueter's patient perished during the operation from laceration of the renal vessels. In one of his cases, in attempting to enucleate the tumor, Czerny tore the renal artery, and was obliged to tie the aorta, the patient surviving ten hours; and in a second case the patient died on the table from bleeding from the lacerated renal vein. Luecke tore the vena cava, but arrested the hemorrhage by temporary compression, the patient expiring from uræmia on the fourth day, the remaining kidney being cystic and contracted.

In expressing an opinion as to the wisdom of extirpating a kidney, the seat of malignant disease, the questions of the immediate mortality of the operation, of the chances of early recurrence, of the prolongation of life, and of a final cure must be considered.

1. As I have pointed out, the mortality of the procedure is 57.14 per cent., which is not only greater by 20 per cent. than that after nephrectomy for other lesions, but also exceeds that of colectomy, laryngectomy, or any other excision for malignant growths, with the exception of resection of the pylorus and abdominal hysterectomy. On this ground, therefore, the operation is to be condemned.

2. The question of recurrence is not easy of solution, since of the nine recoveries, there is no further history in two; one died of recurrence in nine months, the case having been that of a child two years and a half old, under the care of Jessop; one died of secondary growths in forty-five days, the primary growth having existed two years; and five remained well, respectively, for thirty months, twenty-two months, three months, two months, and forty-six days. Hence, the disease returned in 28.57 per cent. of the cases. All of the tumors were sarcomatous. Of the patients who did not survive the operation, in at least four—the growths having been sarcomatous in two and carcinomatous in two—portions of the tumor were left behind, and continuous recurrence would, of necessity, have carried them off. Of the nineteen living or dead cases in which the result is known, therefore, the disease returned or would have returned in six, or 31.57 per cent.

3. Extirpation of the kidney for malignant growths certainly does not prolong life, if the statements of Rohrer and Roberts be true, that the average duration of life without operation is two years and a half for adults, since, of the cases in which this point could be traced, the average life was rather less than twenty-four months.

4. The cases are too few upon which to base the decision of final cure; but the record of two patients who remain well for thirty months and twenty-two months is far from being a gloomy one, in an affection which is invariably fatal if not subjected to surgical interference.

Although the immediate mortality is so great, and although the recurrences number nearly one-third, I am of the opinion that, after the risks have been fully explained to him, the patient should be given the chance if he desires it; and this view is

strengthened by the fact that primary malignant disease of the kidney evinces less tendency to invade the surrounding tissues, and give rise to metastases, than similar disease in other organs. Kühn declares that secondary growths are found in only 28 per cent. of the cases in children, and in 44 per cent. in adults. If these statements be true, nephrectomy, in the early stage of the affection, should, in the future, show more favorable results than have attended it up to the present time, especially as the majority of the growths of the kidney are of a sarcomatous nature.

In the extirpation of sarcomatous or carcinomatous kidneys, the following rules should, if possible, be rigidly adhered to: The capsule of the organ should always be removed; the incision, when the growth is large, should be made along the outer side of the rectus muscle, as recommended by Langenbuch, through which the ureter and renal vessels can be more readily and safely reached than through the median ventral incision; and the ureter should be pinned outside the abdominal incision, as practised by J. Knowsley Thornton, so that the septic material which it contains may not be left in the cavity of the belly. Another real source of danger is sloughing of the ligated ureter when dropped into the abdomen. Such an accident happened to Mr. Thornton, in his case of extirpation of a kidney for sarcoma of its capsule; but, as he informed me in a letter dated April 1, 1883, the slough came away through the drainage opening on the fortieth day, and his patient was alive at the time that he wrote me.

The gall-stone was removed with the investing portion of the gall-bladder to get rid of a source of discomfort in the event of the patient's recovery. That the operation would not increase the risks was inferred from the fact that Langenbuch, in 1882, successfully extirpated the gall-bladder of a man, forty-three years of age, on account of calculous concretions. The stone might have been removed by an incision, which could have readily been united with the continued suture; but the former course was deemed the more advisable, as the weak condition of the patient demanded the immediate closure of the abdominal incision in order that she might be put to bed, and that bottles of hot water might be applied to the extremities.

CLOSURE OF THE JAWS AND ITS TREATMENT,

With the Report of a Case in which Complete Occlusion followed a Gunshot Wound of the Left Superior Maxilla, received at two-and-a-half-years of age, and which was relieved eighteen years subsequently by operation according to a new method.

By J. EWING MEARS, M.D.,
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[Read June 1, 1883.]

Closure of the jaws exists, as is well known, under two forms—the spasmodic or temporary, and the chronic or permanent. The former occurs usually in connection with some condition which produces irritation in the motor filaments of the third division of the fifth nerve, causing spasmodic contraction, notably of the masseter and internal pterygoid muscles. Among the causes may be enumerated delayed or difficult eruption of the third molar or wisdom teeth of the lower jaw, the development of tumors from the external surface of the ramus and body of the lower jaw, alveolar abscess in connection with the posterior teeth, necrosis, suppurative tonsillitis, and finally I have observed it to follow operations upon the lower jaw when performed in the molar region.

For the relief of these conditions the treatment consists in the removal of the causes. When dependent upon the impeded eruption of the wisdom tooth, the mouth should be opened by levers under the influence of an anæsthetic agent, and the second molar tooth should be extracted so as to afford space for the third

molar, or, if the latter is found to be an imperfectly developed tooth, as sometimes happens, the offending organ should be removed. For purposes of mastication, the third is regarded as of less value than the second, and it would seem, therefore, to be better practice to remove it in all cases. Where tumors, necrosis, and alveolar abscesses exist as causes, the treatment is obvious. After operations in the molar region of the lower jaw, antispasmodic remedies should be administered, and water, as hot as it can be borne, should be held in the mouth in contact with the parts so as to allay irritation.

In permanent closure of the jaws we have quite different conditions to deal with. The contraction is not due, in this form, to the perverted function of pre-existing structures, but to the formation of adventitious tissues, which firmly and permanently lock the jaws, and in some forms defy successful removal. Although not a very unusual occurrence, it would appear, from an examination of surgical text-books, that the description and treatment of this truly distressing condition have not, until a recent period, claimed the attention their importance deserves.

In volume iii. of the third American edition of Velpeau's Surgery, edited by Dr. Valentine Mott, of New York, the latter, under the caption of Concluding Remarks, records sixteen cases which came under his care from 1812 to 1843, and in which he operated with great success, accomplishing *perfect cures* in all instances save one. He enumerated three causes upon which "immobility of the lower jaw" depended. The first and most frequent cause was the formation of unyielding cicatrices, resembling, as it were, *adventitious ligaments*. A preternatural rigidity or dynamic contraction of the muscles was stated as the second, and the formation of an osseous plate of bone connecting the upper and lower jaws was given as the third cause. His process of surgical treatment consisted in the use of an instrument constructed upon the screw and lever principle with which the jaws were forcibly separated. The instrument employed was devised by Scultetus, and is depicted in his Armamentarium Chirurgicum. In one case (in 1831) all efforts to separate the jaws were ineffectual, and he regretted myotomy was not known

at the time the operation was performed, for he was persuaded that subcutaneous division of the masseter muscle would have liberated the jaws, and resulted in rendering complete his list of perfect cures. With the expression of high respect for the opinions and statements of this distinguished surgeon, I cannot think that he was afforded the opportunity of examining, at the expiration of a sufficient period of time, the results of his method of operation in the cases recorded.

In the Jacksonian Prize Essay of 1867, Mr. Christopher Heath, of London, recorded cases in which he had performed operations for relief of permanent closure of the jaws, and in a chapter devoted to the subject collated the work of others up to that date. According to Mr. Heath's statements, English text-books on surgery were remarkably barren of any information upon the subject. He alludes to a reference of Mr. Cooper, in his Surgical Dictionary, to a case treated by Dr. Valentine Mott, in 1831, in which an operation was performed for closure of an opening in the cheek caused by sloughing and accompanied by closure of the jaws. In the edition of Cooper's Dictionary, issued in 1861, closure of the jaws is described as occurring after "sloughing of the cheeks and gums from profuse salivation, the cicatricial bands being so rigid as scarcely to allow of the separation of the teeth, but they became more pliant in time." The latter part of this statement, Mr. Heath very justly says, is not borne out by general experience. Due credit is given to our distinguished President, Prof. S. D. Gross, for giving "by far the most complete account of the affection in his large work on Surgery."

Prof. Gross, in the first edition of his Surgery, states, as the most common cause, according to his observation, "profuse ptyalism followed by gangrene of the cheeks, lips, and jaw, and the formation of firm, dense, unyielding inodular tissue, by which the lower jaw is closely and tightly pressed against the upper. In the worst cases there is always extensive perforation of the cheeks permitting a constant escape of the saliva and inducing the most disgusting disfigurement. A second cause is given as anchylosis of the temporo-maxillary joints, consequent upon injury or arthritic inflammation. The formation of an osseous

bridge, uniting the jaws or extending from the lower jaw to the temporal bone, is assigned as the third cause. The effect of the closure is stated to be a serious interference with mastication and articulation, and if it occur early in life it is often followed by a stunted development of the jaw.

With regard to treatment, Prof. Gross states that anchylosis of the temporo-maxillary articulation may be relieved by forcibly depressing the lower jaw with wedges or levers, an anæsthetic agent having been administered. In order to prevent re-formation of the adhesions, the lever is to be used daily for many months or years. When the immobility depends upon the presence of inodular tissue, the proper remedy is excision of the offending substance—an operation which is both tedious, painful, and bloody, and unfortunately not often followed by any but the most transient relief owing to the tendency in the parts to reproduce the adhesions, however carefully and thoroughly they may have been removed. After the excision is effected, the patient must make constant use of the wedge, wearing it for months and years so as to counteract the tendency to reclosure. In a large experience, Prof. Gross found that but few patients were permanently relieved by operations of this kind. Where immobility of the jaw is caused by the formation of an osseous bridge it may be remedied by the removal of the adventitious substance by means of the saw and pliers. Sometimes, however, such a procedure is rendered inexpedient on account of the long duration and excessive firmness of the anchylosis and the large quantity of the new osseous tissue.

In cases of long standing it is recommended to divide the masseter muscle subcutaneously and with great care, lest important vessels be divided. Plastic operations may be performed to close the gap in the cheek which may follow salivation.

I have made these liberal quotations from Prof. Gross's work in order to show how little confidence was reposed in the methods of operation then in vogue in this country at least, and how unsatisfactory the results were which followed their employment.

In 1855, Dr. Friedrich Esmarch, Professor of Surgery in the

University of Kiel, read an essay at the Congress at Göttingen on the "Treatment of Closure of the Jaws from Cicatrices."[1] In this essay he gave an elaborate description of the anatomical relations and histological characters of the mucous membrane of the cavity of the mouth, as well as of the pathological conditions which occur in cicatricial formations in this membrane. He described the outer space, or buccal cavity as it is termed, between the alveoli and teeth and cheek and lips as an elastic dilatable sac, and showed that as soon as this sac shrinks together, loses its elasticity, or is replaced by a rigid substance, the mobility of the jaw must either be impaired or entirely cease. After ulceration or sloughing of the mucous membrane, cicatricial contraction ensues, which the depressors of the jaw cannot overcome. If the cicatricial tissue is entirely excised, re-formation of a cicatrix, possessed, if anything, of greater powers of contraction, takes place. Mechanical appliances, it is true, such as the metal shields of Mr. Clendon, of London, dentist, may retard and limit, to a certain extent, this formation, but it requires their constant use for long periods of time, and under very painful and trying conditions to the patient. In order to supply the place of the destroyed mucous membrane, Dieffenbach suggested, after division and separation of the cicatrix from the bones, the covering of the raw surfaces with sound mucous membrane transplanted from an adjacent part. In the most favorable cases this is almost impossible, owing to an absence of sufficient healthy mucous membrane near by. Jaesche, in 1858, recommended the use of a flap of skin instead of mucous membrane, which may be also difficult to obtain from a favorable point. Esmarch, however, would not hesitate to take a flap of skin from so remote a part as the arm. In view, therefore, of the pathological conditions which exist in these cases, and of the great difficulties presented in overcoming them, as well as the failures which followed efforts made, Esmarch recommended the formation of an artificial joint in front of the contraction, in order to give the other half of the jaw some, although a limited

[1] Die Behandlung der narbigen Kieferklemme durch Bildung eines künstlichen Gelenkes um Unterkieferkiel, 1860.

motion. This joint was to be formed by the excision of a segment of bone of such size as to prevent union of the divided ends, and the operation was performed by an external incision along the base of the jaw.

Although this operation was suggested by Prof. Esmarch in 1855, the method was not employed until 1858, and then by Dr. Wilms, of Berlin.

In 1857, shortly after the suggestion of Esmarch had been made, Prof. Rizzoli, of Bologna, operated for permanent contraction of the jaws by a simple division of the lower jaw in front of the cicatrix, using for that purpose powerful forceps applied within the mouth. In order to prevent union he inserted a piece of gutta-percha between the cut surfaces of bone, which procedure, it is stated, was accomplished successfully. Satisfactory results have been achieved by English and Continental surgeons by the employment of both of these methods. Many years before, Dieffenbach had endeavored to relieve the closure of the jaws by the division of the ramus of the jaw, and in this manner obtain the formation of an artificial joint. This method of operation was not followed by complete success.

In cases where closure results from anchylosis of the temporo-maxillary articulation, Mr. Heath recommended either division of the bone or resection of the joint, giving preference to the former on account of the ease with which it could be performed from within the mouth "by dissecting up the mucous membrane and masseter muscle so as to introduce a narrow saw or strong bone-forceps and dividing the ramus as high up as convenient," and thus establishing a false joint as originally proposed by Dieffenbach for cicatricial contractions.

He also quotes from Sédillot the case of true anchylosis in which M. Grube, in 1863, divided the ramus of the jaw from within the mouth with a straight chisel, and in this way formed a false joint. The masseter muscle was subsequently divided subcutaneously, and the cure was permanent.

In the last edition of his work on Surgery, Prof. Gross refers to the two methods suggested by Mr. Heath, neither of which he characterizes as very promising nor easy of execution. He

reports a case of complete synostosis of the lower jaw on the left side, the result of rheumatism, in a girl seven years of age, in whom, in 1874, he exsected the condyle along with a portion of the neck of the bone, and succeeded in establishing excellent motion. The parts were exposed by a curvilinear incision in front of the ear with hardly any loss of blood, and the condyle was prized out of its socket by means of an elevator which combined the principles of a lever and a knife.

Under the date of December 6, 1845, Dr. John M. Carnochan, of New York, in a communication addressed to Dr. Townsend, translator of Velpeau's Surgery, called attention to the fact that he was the first to put into practice the division of the masseter muscle, and the first to propose simultaneous division of the masseter and temporal muscles of one or both sides, and the formation of an artificial joint on the inferior maxillary, either by simple division of the bone or by exsection of a portion of it as a remedy for immobility of the jaw. In this communication he reports at length a case upon which he operated in 1840. After dividing the adhesions and applying the screw lever, he failed to separate the jaws; he then divided the masseter muscle subcutaneously with a narrow tenotome, and again applied the lever. Again the efforts were without avail, and before resorting to division of the temporal muscle, as was his intention, he reapplied the lever, and under the force employed the jaw was fractured in its body. The patient could now open the mouth to the extent of an inch and a half. The result following this accident, and the successful attempt of Dr. John Rhea Barton, of Philadelphia, to form an artificial joint in the femur, suggested the application of this principle to the lower jaw. With the intention of putting this plan into execution at a future day, the union of the fractured bone was permitted to take place, and the jaw became again immovable, with a slightly increased space between the teeth. So far as I am able to ascertain, the operation suggested was never performed. The inference he drew from the results in the case was that mere fracture or section of the inferior maxilla, even accompanied by repeated and free motion, would be insufficient to produce an artificial joint, and

that to fulfil this indication the entire exsection of a portion of that bone (towards the angle or at some other locality which the nature of the individual case might suggest) would be necessary. It will be seen from this statement of Dr. Carnochan that the operations described as that of Esmarch and of Rizzoli were, in fact, suggested by him some fifteen years previous to the presentation of the essay of Esmarch at the Congress at Göttingen.

A *résumé* of the methods of operation which have been suggested for the relief and cure of permanent closure of the jaws shows that they have been as follows:—

1*st.* Excision, more or less complete of the cicatricial bands or osseous formations, and the subsequent employment, for a long period of time, of wedges and levers to retain the separation of the jaws. Transplantation of mucous membrane to cover the surface of the wound as suggested by Dieffenbach, or transplantation of skin as practised by Jaesche.

2*d.* Division of the cicatricial tissues, and the adaptation of metal shields, not only to prevent re-contraction, but to re-establish the sulcus of mucous membrane at the base of the alveolus.

3*d.* Dieffenbach's method of simple division of the ramus of the jaw—and a formation of a false joint *behind* the point of contraction.

4*th.* The formation of a false joint as originally suggested by Carnochan. Esmarch's suggestion that it be formed in front of the contraction, and that a segment of bone be removed for this purpose—by external incision.

5*th.* The formation of a false joint *in front* of the contraction by simple division of the bone, made by forceps applied within the mouth—Rizzoli's method.

In closure due to anchylosis of the temporo-maxillary articulation, the methods practised are:—

1*st.* Division of the ramus of the jaw from within the mouth, either by saw, forceps, or chisel, and the formation of a false joint.

2*d.* Exsection of the condyle with a portion of the neck, the incision being external.

A patient having presented herself to me for relief from permanent closure of jaws of long standing, due to cicatricial contraction, I had occasion to study the various methods of operation which had been proposed for this form, and became impressed, as the result of this investigation, with the fact that, objections more or less valid, could be urged against each, and that it was possible to carry into execution successfully a method with the hope of securing better results. Moreover, in two cases I had failed by the plan of excision, and the use of levers to accomplish satisfactory results, and I had witnessed similar failures in others. The objections in cases of cicatricial contraction against the methods of operation above enumerated may be stated as follows:—

1*st*. Excision.—The re-formation of the cicatrix and the great pain to which the patient is subjected in the use of wedges, levers, and screws—the difficulties of securing flaps of mucous membrane and skin from adjacent parts and their successful transplantation. The almost universal failures.

2*d*. Division and Use of Shields.—The pain and inconvenience experienced by the patient in the use of the shields, and difficulty of obtaining the full coöperation of the patient in carrying out the necessary manipulations within the mouth.

3*d*. Division of the Ramus *behind* the Contraction.—Dieffenbach's Method.—The difficulty of obtaining a permanent false joint after simple section of the bone, and without division also of the overlying masseter muscle.

4*th*. Carnochan's Method as practised by Esmarch.—The loss of one-half of the jaw for the purposes of mastication, where excision is made in front of the contraction. Its inapplicability when both sides are affected, and the deformity which results.

5*th*. Carnochan's Method as practised by Rizzoli.—The difficulty in accomplishing the formation of a false joint by simple division of the bone—the tendency to reunion being much greater than when a segment is removed.

Considering these objections I decided to operate upon my patient in the following manner: By division of the ramus of the jaw, about its middle, exsection of the condyle and division

of the insertion of the temporal muscle, thus releasing the coronoid process and effecting its removal with the condyle—division of the masseter muscle at its points of origin—non-interference with the cicatricial band. By this plan I hoped to secure sufficient space for free movements of the remaining portion of the ramus, and I proposed to utilize the cicatricial band as a *quasi* ligament, and obtain movement of the bone between this band and the internal pterygoid muscle. By division of the masseter at its point of origin, I proposed to relieve the tension of this muscle and more effectually prevent union of the divided fibres.

Case.—The patient consulted me in January of this year (1883), and gave the following history of her case: She is now twenty years of age. Eighteen years ago, when two years and a half old, and then residing on the banks of the Brazos River in Texas, she received a gunshot wound of the left superior maxilla, the charge, medium sized bird shot, entering just below the inferior margin of the orbit.

At the time of the receipt of the injury she was engaged in play on the porch of her dwelling, and the gun was discharged by a lad thirteen years of age, whose height compelled him to hold the weapon at an angle in taking direct aim at her head. This was done in play, the gun being presumed to be unloaded. The father, who was soon at her side, made efforts to check the hemorrhage which ensued, and sent immediately for the nearest physician, living some thirty-two miles distant. Late in the evening he arrived, "tired and cross," and declined to do anything, as he did not like to cause unnecessary pain, and as the child was sure to die. After a night's rest he returned home.

On examination it was found that the charge had entered just below the left orbit, comminuting the upper and fracturing the lower jaw as well, passed through the mouth and emerged below the left ear. The father, in the hope of saving his child's life, continued his efforts to ward off fever and allay swelling by the administration of simple remedies, and the application of lint saturated in arnica. At the same time the mouth was forced open to cleanse it, but so great was the pain caused by the effort, that it was discontinued. The patient was, after a short time, unable to open the mouth for the purpose of taking nourishment, and finally all efforts

were abandoned to effect the separation of the jaws, and soon they became firmly locked. At the expiration of a week following the accident, another physician was summoned, who removed some wadding, shot, and pieces of bone, and gave a very unfavorable prognosis as to the recovery of the child.

At the time of the accident the globe of the eye did not appear to be seriously injured, but the inflammation which attacked the parts and the cicatricial contraction which ensued in the closure of the wound, destroyed vision, and drew down the ball to such an extent as to expose but a line of the iris, and produced a marked condition of ectropion. Soon the patient became accustomed to eat only soft food, which she learned to force between the teeth. The pressure exerted upon the teeth caused them to break, and the posterior teeth, which could not be erupted, became carious and gave pain.

A careful examination of the parts involved showed firm and complete occlusion of the jaws, absence of the two central and right lateral incisor teeth of the upper jaw, which had been worn away by the pressure made in rubbing pieces of food over them. Slight lateral movement could be obtained, showing absence of temporo-maxillary anchylosis. On passing the finger into the buccal cavity of the left side, a dense, rigid, cicatricial band could be felt extending from the molar region of the upper, to the molar region of the lower jaw. Eight to ten of the anterior teeth of the upper and lower jaws had fully erupted; the remaining were imbedded in the alveoli of the jaws, the edges of the crowns being seen in some instances. Just below the border of the orbit, on the left side, occupying the position of the upper portion of the canine fossa, there was a deep sulcus, lined by integument which had been drawn into it, and with it the lower eyelid; complete eversion of the lid having been produced, and the lower segment of the globe being constantly uncovered. This sulcus was caused by the entrance of the charge of shot. There was marked flattening of the entire left side of the face produced by the injury inflicted upon the structures, and the absence of subsequent full development. On the left side of the neck, on the upper portion, a few shot could be felt lying beneath the integument, and one was also felt beneath the mucous membrane overlying the left side of the lower jaw. Articulation was somewhat impaired.

On the 26th of January, 1883, I operated upon the patient in the manner above indicated, and with a view of relieving, by the one

operation, the condition of ectropion. To accomplish this, I began the incision to the nasal side of the sulcus, carried the knife through it, and continued the incision along the lower border of the zygomatic arch to the tubercle at its base. I then dissected up the tissues from the bottom of the sulcus, and also from the anterior portion of the floor of the orbit, in order to release them completely, and thus replace the lower lid. Owing to the contraction which had ensued, it was found necessary to divide this flap of tissue transversely, bring it into proper position, and secure the edges by a hare-lip pin. Continuing the dissection, I divided the fibres of the masseter muscle separating the entire origin, and then depressed it with the superjacent structures, thus very easily and completely exposing the coronoid process, the articulation and outer surface of the ramus to one-half its extent. With strong bone forceps I divided the ramus, severed the ligaments of the articulation with the probe-pointed bistoury, then twisted out the articular head, and finally detached the insertion of the temporal muscle, removing the piece of the ramus with the processes which I exhibit (Fig. 1).

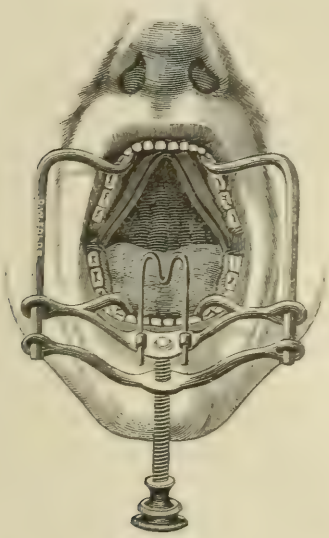
Fig. 1.

Efforts were now made with wedges of wood to separate the jaws, but without success. On passing a probe between the jaws I found a strong osseous band which firmly united them; this I divided with the Adams saw, passing it between the imperfectly erupted teeth, and sawing from before backward. The wedges were again used, and the jaws separated to the extent of one inch between the processes at the position of the incisor teeth. The divided portions of the osseous band were then cut away with pliers and knife, and found to consist of a thin plate of bone.

On recovery from ether, the patient found no difficulty in opening the mouth, although it was accompanied by slight pain. In a

few days the pain disappeared, and the mouth could be readily opened. Within three weeks after the operation twenty-one roots and partially erupted teeth were extracted, so as to fit the processes for the adaptation of artificial dentures. The incision was closed by four interrupted silver sutures, and dressed with lint saturated with carbolized oil. In order to assist in the exercise of separating the jaws, I employed daily for two weeks a mouth gag, which I devised some years since, and which works with a strong screw. (Fig. 2.) In using it the patient complained of pain on the sound

Fig. 2.



side, the structures of which had become somewhat rigid owing to long disuse.

At this time the patient is wearing a set of artificial dentures, which are retained readily in place, and which serve the purpose of mastication excellently well. The ectropion is relieved so that the globe is covered by the eyelids. Articulation is much improved. No tendency to re-contraction is observed. On the contrary, the patient states that her ability to open the mouth increases each day, and that she can separate the jaws to a greater extent than was done at the operation, and without pain. Exact measurement gives the

distance between jaws, when separated, as one-and-a-half inches at the position of the incisor teeth.

Whilst, in the present case, the incision was made so as to relieve the ectropion as well as to uncover the joint and ramus, I should modify it but slightly in other instances, as it completely exposes the parts, and does not involve either large branches of the facial artery or nerve, thus avoiding excessive hemorrhage or subsequent facial paralysis. It should begin at the middle of the lower border of the malar bone, and be carried outward in contact with this border and the inferior border of the zygomatic arch, to a point over the tubercle at its base; it is desirable to keep near the border of the zygoma in order to avoid wounding the duct of Steno, which lies a finger's breadth below.

If there is objection to the disfigurement produced by the cicatrix, when the incision above described is employed, the ramus can be exposed and divided, and disarticulation accomplished by an incision beginning about the middle of its posterior border and carried along the angle and base of the body of the jaw to such an extent as to permit easy elevation of the flap. The position of the facial artery is to be remembered, and section of this vessel avoided if possible. Branches of the facial nerve will be divided, and more or less facial paralysis will follow. The parotid gland should be pushed up so as to escape injury.

So far as I am aware, the plan of operation suggested and practised is novel, in the fact that it includes removal of both coronoid and condyloid processes with the upper half of the ramus, as well as division of the masseter, external pterygoid, and temporal muscles, at the point of origin of the former, and the insertion of the latter. The advantages claimed over other methods are :—

First. Its application to all forms of permanent closure, that due to temporo-maxillary anchylosis, as well as to cicatricial formations.

Second. The utilization of the entire body of the jaw in opening the mouth, not only affording in this way greater advantage in mastication and articulation, but serving to prevent deformity.

Third. The formation of a more perfect artificial joint in the removal of both processes, thus overcoming the resistance of the more or less fixed upper segment, when the joint is made either in the body or the ramus of the bone.

DISCUSSION.

THE PRESIDENT. It is said that women talk too much, and that when their jaws are locked it is a good thing to let them remain so; but I am of the opposite opinion, and believe that when we can unlock them it is our duty to do so.

Dr. HUNTER MCGUIRE, of Richmond, Virginia.

I think the Association is under obligation to Dr. Mears for this interesting essay. I cannot agree with him, however, that resection of the jaw is always necessary. I had occasion once to resect the jaw for anchylosis of the temporo-maxillary articulation. The operation was performed in a child which had inflammation ending in the production of redundant callus with complete anchylosis, that resisted all effort to overcome for months. I took out a large wedge-shaped piece of bone just at the joint, with the view of making a false joint. The operation was, of course, easy; the piece removed was large, and the result was for the time gratifying. The child could open its mouth an inch and a half, and thus far did well. Two months after this the child complained of pain in the opposite joint. This continued, and no persuasion upon the part of the father, mother, or myself could make the child keep up the motion of the jaw. The pain was so great that the child absolutely discontinued the use of the new joint I had made, and the result was that complete re-union took place. I just mention this; I do not attempt to explain how it was, whether it was from sympathetic irritation or not—I simply state the fact. With regard to cutting the cicatricial bands and cutting loose the osseous plates, I had a case in one of the hospitals of Richmond in which I removed the osseous plates, cutting through the cicatricial bands, six years ago, and the woman has complete use of the jaw; and I think nobody could

observe anything different from that which is natural in the appearance of the jaw, except a want of development from the lack of use for a long time before my operation. Several cicatricial bands existed, and there was, so far as I could see, no possible motion of the joint. One of the most interesting points was the complete restoration of motion after its absolute absence for ten years. That woman got well, for the reason that I taught her to use the wedges and levers herself, and she persisted in their use for months and years. But for the last year she has stopped their use, because there is no occasion for it. And I believe that this is the great trouble in such cases, that the stretching and dilatation is not kept up long enough. Thus, in cases of stricture of the urethra, we expect to effect a cure only after continued dilatation. In a stricture anywhere, the contraction of the cicatricial tissue is one of the worst elements, and dilatation must be kept up for a long time.

Dr. Alfred Post, of New York, N. Y.

As I have paid more attention to the subject of cicatricial contraction, probably, than any other surgeon living, I can, perhaps, speak with some authority upon the subject, although not with special reference to closure of the jaws. When cicatricial contractions are capable of yielding a slight motion, efforts at overcoming the contraction, often repeated and long continued, may do good. But, when you start with the case where there is no motion, and the patient is not able to bear the continuous efforts at overcoming it, incision is necessary. The mode that I use in making the incision differs from that used by most surgeons. Instead of making one incision, and keeping the parts wedged apart, leaving an opening to be filled up with one mass of cicatricial tissue, I make several cuts so as to divide the cicatricial mass into small rhomboidal segments, and I find that there is scarcely any limit to the extent to which these incisions may be made through the cicatricial tissue, if you do not raise the mass from its base. I never knew sloughing to occur where the mass was left attached to its base; but if you raise the whole mass from its base, it will almost invariably slough. I have tried this method several times, making the incision down to the soft, normal tissues beneath, and then applied force to tear the parts asunder, and I have had very good results. I find that the

healing occurs in a few days, and instead of having a large mass of cicatricial tissue capable of exerting great force, we have a number of small masses capable of exerting but little force. It is necessary, of course, for a long time to keep the parts on a stretch. I have demonstrated in several cases, that where the contraction was of the worst character, producing absolute or almost absolute immobility, where I have succeeded by long-continued stretching and active exercise, a period arrives—usually after several months or a year—when all efforts at contraction cease, and the parts gain their mobility.

A case that came under my treatment is very instructive, because it occurred a long time ago. A child was brought to my clinic in the University Medical College, suffering from a burn. The hand was drawn back against the arm, and union had taken place in that position. I detached the hand from the forearm and kept up the treatment for eight months, attending to it myself, and during the greater part of that time making daily visits. There was an enormous surface exposed. Of course, in the early treatment of the case, I resorted to all the means of keeping down the granulations, but finally the wound healed while the hand was flexed in that position, and the operation became necessary. That child has now grown to be a woman, and is a very skilled performer on musical instruments, using those fingers in performing on the piano. The mobility has become perfect. The strength is not quite equal to that of the other hand, but she has perfect use of it. I think these are very important matters in connection with the treatment of contracted cicatrices, and I always maintain the treatment as long as any disposition to contraction exhibits itself. But I am satisfied that if the treatment is thorough and long enough continued, all disposition to contract is finally relieved.

Dr. W. T. Briggs, of Nashville, Tennessee.

I have been so unfortunate, on account of the free administration of mercury in our part of the country in former times, to meet with many cases of anchylosis of the jaws. Some of them I relieved by levers and plugs, with a great deal of pain and suffering. Some I relieved by bony exsection, and in one case by bony incision, but nearly all of them show a disposition to contract, and in some of them the contraction is as great as before the operation. Last

summer, however, I met with a case, in a young man of fifteen or sixteen years, who had fallen and struck his chin against some hard substance, producing an abrasion which was followed by inflammation of the jaw and arthritis of the joint, which went on until complete immobility resulted. The wound was received when he was four years of age, twelve years before I saw him. He had been under the treatment of several physicians, one of whom thought he had succeeded in overcoming the immobility. But it gradually returned until it became almost as bad as before, with complete bony anchylosis of both joints. His jaw, of course, was not developed, falling back behind the other jaw an inch or an inch and a half, and his lower teeth had grown up into the upper gum. On several occasions, in becoming nauseated and in the efforts at vomiting, he almost strangled from the inability to get anything out of the mouth; and it was almost as hard to get anything in, and he was becoming very much emaciated. When he presented himself to me, I considered all the plans of resection, removal of the condyle, resection of the bone and section of the muscles; but, unfortunately, he had no muscles left, and he could not have moved his jaw if I had relieved the contraction. Then came the question of a serious operation and doubt in my mind whether he would have any benefit if I could succeed in keeping the mouth open. Regarding that the main object of the treatment was to obtain an avenue to his stomach, I determined to operate with that intention. I made an incision along the base of the jaw, raised the flap, and divided the bone on each side as far back as the first molar tooth, removing in this manner the anterior portion of the body of the jaw. The temporo-maxillary articulations were still anchylosed, and I could not break up the adhesions. He could readily swallow fluids. All his powers of mastication had gone, but he got along very well, and the result was considered very good.

Shortly after, another patient was brought to me with the same condition, produced by injury to the jaw. I proposed to perform the same operation in his case, but he declined, and has never yet been operated upon.

Dr. W. W. Dawson, of Cincinnati, Ohio.

I would like to ask Dr. Mears a question. If after dividing the ramus in this way, is it then necessary to exsect the head of the

bone? Is not the division of the bone the principal factor in establishing motion? It need not be said that the dissection necessary and the consequent damage to the neighborhood in removing the condyle is by no means insignificant.

Dr. Mears. I am very glad to have heard the report of the very interesting case by Dr. McGuire, and the success of his treatment; but I cannot but believe the case which he reports is a strong argument in favor of the method I have employed, *i. e.*, resection of the coronoid and condyloid processes; from the fact that by this method the mouth is again opened and prepared for use in a very short time and with but slight pain to the patient. He says his result was obtained after months and years of use of the wedges and levers; now if after a few days we can secure opening of the mouth and the use of the jaw without pain, that is a result to be desired. If, as I understood from the remarks of Dr. Post, cicatricial tissue can be converted into an elastic structure—I hardly believe that it can be used as such. We try, however, to get motion in cases of anchylosis due to cicatricial bands by division of the structures outside of the cicatrices. We get motion by the elasticity of parts outside of this tissue, but not in the tissue itself. We then call upon the adjacent mucous membrane, if there is any present, and this is the point Professor Esmarch made in his paper: That, so long as there is any mucous membrane present, adjacent to the cicatrix, so long could we get movement, but as soon as there is sloughing of the entire mucous membrane, there is no possibility of getting movement in the cicatrix. Transplantation of mucous membrane has been suggested as stated, but is usually impossible to accomplish this successfully.

With regard to Dr. Briggs's case: The operation performed by him must, of necessity, be followed by a great deal of deformity; and, while this was better than the original condition, and afforded relief to the condition, it is, to my mind, a method which should be resorted to only in cases where none other could be employed. I should have been tempted in that case to have taken out the whole jaw by the subperiosteal method, with the expectation of having a reproduction of bone sufficient to support an artificial denture. The deformity would not have been much greater than after removing a portion of the body of the jaw, and I believe there would have been motion developed.

In reply to the question of Dr. Dawson, I desire to say that simple division of the ramus is not sufficient to secure an artificial joint—union it may be said invariably takes place. The important point in the method of operation I have suggested is that by the removal of the upper segment of the ramus, including the condyloid and coronoid processes, no bone is left with which re-union can occur; a permanent false joint is obtained and ample space is afforded for the movement of the lower segment of the ramus without resistance on the part of an opposing segment of bone.

REMOVAL OF MECKEL'S GANGLION FOR THE RELIEF OF TRIFACIAL NEURALGIA,

With Report of Cases.

By A. VANDERVEER, M.D.,
SURGEON TO THE ALBANY HOSPITAL, ALBANY, NEW YORK.

[Read June 2, 1883.]

In the paper which I am about to read I do not know that I shall present anything especially new or original, and yet it seems to me the cases are worthy to go on record as illustrating the good resulting from operative surgery when medicine has failed to afford the relief sought by suffering humanity.

Mr. S., aged fifty-eight, came under my observation May, 1870. He gave the history of having suffered for over ten years from severe neuralgic pains about the right side of the face, along the alveolar process of the upper jaw and the right side of the tongue, also in the track of the inferior dental nerve. In the preceding five years Dr. Detmold, of New York, as well as the late Dr. Alden March, of this city, had each operated twice by loosening the attachment of the cheek from the alveolar process, affording each time a few months' relief to the patient. I did the same operation, but in September, 1870, the pain returned with as much severity as ever. I then made use of a very thorough treatment of iron, arsenic, Brown-Séquard neuralgic pills, injections of chloroform, and every treatment known to me, but with no especial benefit. In March, 1871, I made a section of the infra-orbital nerve, which resulted in affording great comfort to the patient, and for two years he remained quite free from pain. It then returned, more especially along the course of the infra-orbital, and I suggested the removal of Meckel's ganglion; but his health was such that neither he nor his friends thought it safe. The old gentleman has lived on, however, and, although in

great pain for months at a time, he will occasionally improve, and is alive at the present time.

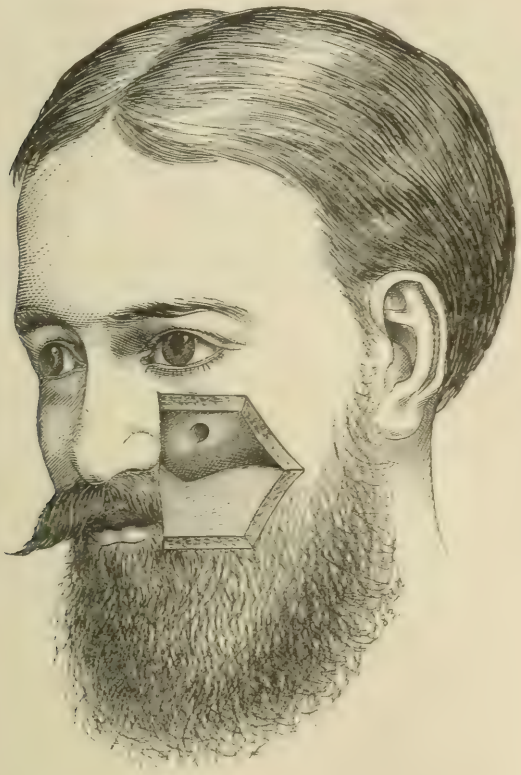
Miss B., aged forty, first consulted me July, 1875, giving the following history: For the past three or four years she had suffered from neuralgic pains in the right side of the face, differing as to length of duration and degree of severity. Her general health up to that time had been good. No serious sickness, and family history clear. Passed the menopause about two years before. Since that time she had had very little relief from pain, unless under the influence of medicine. All of her teeth had been removed (one or two at a time) from the right side of the upper jaw, and some from the lower jaw of the same side, but with little if any relief resulting. For the past six months there had been no remission of pain, although taking large doses of morphine, chloral, and bromides. Had been at times stupid from the effects of these drugs, and yet complaining of pain. Countenance sallow, pale, and features expressive of great torture. She was unable to do any work, read, or become interested in anything. The pain would start in the upper jaw, extend over the face, pass down around and through the lower jaw to the chin, and along the right side of the tongue, also penetrate the temporal region, leaving a sensation of heat in the mouth so severe that, at times, she would be unable to take a drink without having the pain aggravated for hours.

She was thoroughly miserable and despairing of ever getting any help. An examination of the urine showed the kidneys to be healthy. She was unable to give a satisfactory reason for the development of the original trouble, except that she had for the most of her life been confined indoors as seamstress or forewoman in a large store. I gave her large doses of quinine, carbonate of iron, also Brown-Séquard neuralgic pills, but with no apparent benefit. Gave hypodermic injections of chloroform, with a few minutes' respite from pain, but the inflammation following was very severe. I also gave morphine in the same manner, but with so little good resulting that she readily consented to an operation.

On September 5, 1875, assisted by Drs. W. H. Bailey, N. L. Snow, Lewis Balch, and F. C. Curtis, and my two medical students, ether being given, I proceeded to remove the infra-orbital nerve and Meckel's ganglion in the following manner: Making an incision from the angle of the eye down to the bone along the nose for

a distance of little more than inch, then another incision, similar in length at right angles, under the infra-orbital ridge, raising the flap and periosteum, I exposed enough of the anterior wall of the antrum to admit the application of a good-sized trephine, removing a button of bone so that the upper edge of opening exposed the superior maxillary nerve in the infra-orbital canal (Fig. 1). Lifting the

Fig. 1.



Showing Incisions made to expose Infra-orbital Foramen.

latter from its bed by means of the bone chisel, grooved director, and probe, I followed it until reaching the posterior wall of the antrum, where, by means of a smaller trephine, another button of bone was removed, and the spheno-maxillary fossa was reached. The ganglion was now lifted from its bed, and with curved scissors

the nerve and it were severed and removed. This was followed by a sharp hemorrhage, at first somewhat alarming, but controlled by firmly applying portions of sponge to which a silk ligature had been

Fig. 2.

Instrument used to divide the Nerve in Spheno-maxillary Fossa.

tied. The wound in the face was then closed by interrupted sutures, a drainage-tube, with the ligature attached to the sponge, being placed in the most dependent point. The patient rallied nicely from the anæsthetic, and was immediately relieved from all pain. There was considerable trouble in removing the sponge, and the parts suppurated freely, but ultimately healed kindly.

From that time on she has been in perfect health, increasing in flesh, and attending fully to her work, and a more grateful patient I have never seen.

Mrs T., widow, aged thirty-seven, no children. Excellent family history. When two-and-a-half-years old she suffered from a perforating ulcer of the right eye, resulting in the loss of sight and partial flattening of the cornea. At about the age of fourteen she began to wear an artificial eye (she and her family thinking it improved her appearance), and this she continued to wear for seventeen years. During this time she did, in addition to her ordinary school duties, a great deal of fancy needlework, such as embroidery. Always felt very proud of the sight of her left well eye. Could see fine points at a greater distance than many of her friends. During the latter part of the period of wearing the artificial eye, she occasionally noticed that a strong light would give the eye a tired feeling, followed at times by slight pain, and then she would remove it for a little while. The ball did not shrink perceptibly until some time after marriage. After the artificial eye had been out for a short time she would suffer, for a few moments, a slightly increased pain on reintroducing it. Six years ago, when in her usual health, she caught a bad cold by being out in the chilly night air, and then the eye began to pain her severely. The spasms of pain would be only four or five daily at first, confined mostly to the eyeball, and com-

pelling her to leave the artificial eye out most of the time, but there was really no rest for any lengthened period.

She now consulted the late Dr. Robertson, of this city, who advised very positively to let him remove the eye at once, believing it would stop the spasms of pain and prevent any injury occurring to the well eye. This she most decidedly declined to have done. She contented herself by using a soothing eye-wash.

During the fall of 1877 and winter of 1878 the pain was yet confined to the eye, but, in the spring of 1878, it began to extend out over the cheek, down along the lower jaw and right side of the tongue and back over the temporal region.

In the course of the next two or three years she consulted a number of physicians, both in Albany and in New York, some of whom advised the removal of the eye, some an operation on the nerve, and some thought no operation could do any good. All gave her a thorough course of medicine, which she tried faithfully, as well as the use of electricity, and many outward applications. No permanent good resulted; at times she would be better, but the pain would always come on more or less severe just before her menstruation. She was referred to me by my friend, Dr. D. H. Cook, of Albany, in the spring of 1881. I advised her to see Dr. Merrill, also of Albany, and learn as nearly as possible how much the eye had to do in causing the trifacial neuralgia, and whether its removal was not now the best treatment. She saw Dr. Merrill, who advised the removal of the stump, as the eye had now shrunken to quite an extent, but again she positively declined. The Doctor fitted her with another artificial eye, but no marked benefit resulted; she could wear it very little.

I now gave her a very thorough and careful treatment with quinine, iron, Brown-Séquard neuralgic pills, arsenic, and aconitia, but with only slight improvement. The spasms of pain were agonizing to witness, returning every one, two, or three minutes, until at last she got no rest, even with large doses of morphine by stomach or by hypodermic injection, nor with chloroform by the latter method.

Her life was becoming a burden to herself and one of intense sorrow to her friends. She obtained no rest during nearly the whole month of July. August 2, 1881, I consented to remove Meckel's ganglion, though feeling firmly convinced that the proper thing to do was to extirpate the eye. The operation was done in the same manner as described, with the assistance of Drs. Snow,

Merrill, and Worden, no embarrassing hemorrhage occurring. For a few days after she suffered occasional spasms of pain, but on the sixth day she was entirely free from them, and slept for the first a quiet, restful sleep. She remained in a very comfortable condition, improving in general health, not taking any medicine until the last week in November, 1881, when the pain returned along the course of the inferior dental nerve, the spasms being very intense, and coming on so rapidly as to prevent any rest.

I tried faithfully aconitia (Merck's) but with little if any good resulting. She could not bear it in larger than $\frac{1}{80}$-grain doses, three to four times daily.

On January 2, 1882, I trephined the lower jaw, removed, as I thought thoroughly, a section of the inferior dental nerve, giving almost immediate relief. From this time on she remained in excellent health, gaining in flesh and strength, feeling very happy, but gradually losing the sight in her left eye, until the last week in December, 1882, when she suddenly had a spasm of pain along the lower jaw and outer portion of the temporal region. This was followed next day by two or three spasms; the next week she had five or six bad spasms daily, then the pain came on as of old, except over the cheek. She was now very much discouraged, and refused all medicines. Her sufferings became so great, and expecially along the course of the inferior dental nerve, which I am convinced re-united, that, at the urgent request of her family, she finally consented to the removal of the eye, which was done by Dr. Merrill, April 3, 1883.

After this last operation the pain lasted for ten days, then ceased, and since she has been very comfortable. Occasionally she feels a weak sensation about the face, peculiar and difficult to describe, just previous to her menstruation. Says her face feels as though it would fall to pieces. At the present time it is difficult to believe so many operations have been performed about her face, so perfect has been the healing process. In considering this case, I am strongly impressed that the diseased eye was the cause of the reflex irritation.

The next case coming under my observation was that of Mr. H. N., farmer, aged sixty-eight, married; referred to me by my friend, Dr. I. I. Buckbee, of Fonda, New York. He was admitted into the Albany Hospital, March 20, 1882.

About thirty years previous, while working with a machine, the patient received a severe blow over the left malar bone with a stake with which he was trying to stop the machine. This he thought to be simply a bruise, and it apparently soon passed away. A few years later he had ulcerated teeth on that side. For about fourteen years he noticed a dull, heavy sensation in his forehead on the left side, and this was followed in a short time by sharp darting pains under the eye, which could be started by touching the part. He dreaded wiping his face with towel or handkerchief, as it would result in a spasm of pain. The pain would begin with a jerk, sometimes at the nose, and at other times about the eye. Later, it would come on of itself, and was more severe in cold weather.

About ten years ago he began treatment, and was given arsenic, belladonna, opium, etc.; but received no relief.

Five weeks previous to his admission, I saw him for the first time with Dr. Buckbee, and advised a thorough course of treatment with aconitia, which was tried, beginning with $\frac{1}{100}$ gr. four times daily, increasing until $\frac{1}{50}$ gr. was given as often; but no benefit whatever resulted; patient noticing only slightly the physiological effect of the drug. Merck's preparation was used. Mr. N., like Miss B., had had one tooth after another removed, but no relief occurred as the result.

March 20, 1882, assisted by Drs. Snow, Ward, Nailes, Mosher, Wood, Kennedy, and Schofield, I did the same operation as in the last two cases, and with immediate relief. After recovering from the anæsthetic he did not have a single spasm of pain.

His surprise and delight were pleasant to witness. So rapid was his recovery that he left the Hospital six days after the operation with the parts nearly healed. Dr. Buckbee has since informed me that the wound was entirely healed in a few days, and that he has been free from all pain since. His only regret is that he did not have the operation done many years sooner.

When we contrast the treatment of the cases of Mr. N. and Miss B. with that of Mr. S., we can come to but one conclusion: that the removal of the ganglion is really the only true operation to do, and that the manner of making the incisions leaves so little disfigurement as not to deter any patient from submitting to it.

DISCUSSION.

Dr. H. F. Campbell, of Augusta, Georgia.

I recollect, a great many years ago, that Dr. Carnochan reported a number of cases in which he suggested the removal of Meckel's ganglion as a very great improvement upon the operation which had been employed. He considered the removal of the ganglion as a very important element in the treatment. I do not know whether Dr. Vanderveer regards its removal as important in effecting a cure. But I should consider the removal of the nerve as sufficient, without the removal of the ganglion. I do not see how the removal of a centre of vaso-motor action could have a great deal to do with the cure of pain. The ganglion receives some branches from the second division of the fifth nerve, it is true, but I cannot see how the removal of the ganglion can have much effect in the operation. I can speak favorably, however, of the removal of the nerve, for it is a splendid operation for the cure of almost all forms of otherwise incurable neuralgias with which the surgeon has to deal. Whether it is due to a diseased eye, or due to a contraction of the bony canal through which it passes, it is a form of neuralgia, and cutting alone does not cure it. In the Carnochan case, he said that he had cut and dragged out all of the nerve, and the pain was not relieved. In Dr. Vanderveer's case, after removal of the ganglion, the disease attacked the *nerve* and re-appeared. He may not have removed the entire nerve, and, as Brown-Séquard has shown, nerves are reproduced, and the divided ends are reunited. Whether this nerve was reproduced, or there was a portion left uncut, I cannot say. I fully agree with the author of the paper, that there are cases in which this operation, desperate and sometimes dangerous as it may appear, is the only treatment for this form of neuralgia.

Dr. P. S. Conner, of Cincinnati, Ohio.

I do not know of any subject presented to this Association which is more interesting than this one. My attention, several years ago, was directed strongly to this matter, and I had occasion then to investigate the history of every case which had been up to that time operated upon. My own belief was, on theoretical grounds, that the removal of Meckel's ganglion was unnecessary; that there was no reason in the anatomical relations or physiological action of the

ganglion why it should be of any benefit. The operation of neurectomy, it seems to me, would do just as much good as this one, which Dr. Carnochan devised, regarding the ganglion as a "reinforcing battery." There is no reason whatever for such an opinion. The few sensory branches of the ganglion are not sufficient to account for the trouble, simply passing through, as they do in large part at least. The operation does good always, because it is one of some severity. It is often associated with a considerable, and in some cases, frightful loss of blood—sometimes very little. For weeks or months it must do good to the patient, just as he would be benefited by any operation made about the face; but in every single case that I have been able to learn the history of, a recurrence took place afterward, except in Dr. Schuppert's case, in which he afterwards divided the seventh nerve as it came out of the stylo-mastoid foramen. Cases are reported too early for the proper result to be known.

I cannot see, again, what reason there is for us to go clear back into the fossa. We get every benefit from the operation without this, except the division of the posterior dental branches, given off before the nerve gets into the infra-orbital canal. Experience shows that there has been a lessening of pain and great relief afforded by this. Neuralgia of the nasal fossa, I believe, is unknown in connection with these cases.

A large portion of the nerve is removed, it is true, but the result generally is no better than cutting out a portion of the nerve in the infra-orbital canal, with or without additional removal of a part of the supra-orbital branch. In a case in which I removed these two, there was no return of the pain up to the time of the patient's death, two years after.

My colleague, Dr. Dawson, gave considerable relief from an operation on this nerve, and afterwards on the supra-orbital. Any major operation will give relief, but in most cases a return occurs. These cases are always due, I think, to central changes. In many of them the central lesions ultimately carry off the patient, showing that the condition depended not upon a peripheral change, but a cerebral condition. Still, operative procedure is justifiable; and if I had a "tic," I would have my whole face cut into pieces every six days, if necessary to control the pain. But I think it is best to let Meckel's ganglion alone.

Dr. W. W. Dawson, of Cincinnati, Ohio.

I have had a somewhat liberal experience in the surgical treatment of *Malignant Neuralgia* of the face—in some instances the result has been gratifying. Again, the suffering was relieved but temporarily—the disease not radically removed.

In one of my cases, a young man less than thirty, suffering from neuralgia of the second division, eight years have intervened since the operation was performed, and no trace of the affection remains—not one symptom has recurred. I made the operation thorough, extirpating both nerve and Meckel's ganglion. I tunnelled the orbit and broke through the wall of the antrum behind, and divided the nerve just as it emerged from the foramen rotundum. In this case I had a troublesome hemorrhage from the extreme limit of the field, the blood welling up, obstructing, and protracting the operation. This patient had taken large quantities of whiskey and morphine—had used them for years. After he recovered from the effects of the operation his general health improved slowly but surely.

A gentleman of this city, a leading manufacturer, suffered more than any human being I have ever known. To say that he has suffered the torments of the damned would be putting it rather mildly; first, with neuralgia of the second branch of the fifth pair, the superior maxillary, then with the same disease in the inferior dental nerve; after the ablation of both of these, the disease manifested itself in the supra-orbital branch. A painful cicatrix followed one of the operations. After the removal of the inferior dental branch of the fifth pair he was left with a fearful neuralgia of the lip, localized, occupying the portion on the affected side. The first operation for facial neuralgia was made by Dr. Kearns, of Covington, Kentucky. He divided the nerve as it emerged from the infra-orbital foramen, and exsected a portion of its trunk. The patient was relieved for a time, but his sufferings recurred. He then consulted me, and I removed the stump of the nerve from the infra-orbital foramen to the foramen rotundum, and with it the ganglion. The wound healed promptly; he returned to his business in absolute comfort. This surcease was of limited duration, pain in a most aggravated form appeared in the inferior dental. So entirely relieved was the region supplied by the second division that he was anxious to have the newly offending nerve exsected. This I did, and positive relief followed again. The nerve was removed from

the point of entrance to the mental foramen. Some months of comfort intervened, with the exception of an irritable cicatrix, when he again presented himself, complaining of the supra-orbital nerve. This was removed by dropping the skin of the supra-orbital region over the eye, and tracing the nerve back through the notch—a notch as is known sometimes takes the place of the foramen; nearly one inch of the trunk was removed. This wound, like the others, healed promptly.

He returned to business, and seemed fairly well; but, in about six months he complained of a racking, torturing pain in the upper lip—the pain was to the left of the nose and localized; the affected region could be covered with a dime, that is, the point from which the pain seemed to radiate was so circumscribed. He said, pointing to the painful locality, "take this out and I will be well." A portion, about one inch in breadth, was removed from the lip, and after this operation he was for a time absolutely free from pain. He was confined to his house but a short time from the resection of his lip, and he again returned to his factory.

His face, after all of these operations (three exsections of the nerve, the removal of a painful cicatrix, and the resection of the lip) was but little disfigured—so promptly did the parts heal, and so complete was the restoration. His face has remained very tender, sensitive to the touch or to motion of his lip; he will not trust himself to talk, so liable is the movement of the lips to give him pain; but this hyperæsthesia responds most painfully to cold, to a draught of air, or even moderate temperature. This condition of the face increased so that he found most comfort in his room, and in a recumbent position. Here, on his couch, he has remained for several years, comfortable only when in this absolutely quiet condition. He manages his business, and is amused by his family reading to him. This passive life, however imperfect, is a great improvement on the torture and torments from which he has been rescued. He writes upon his slate of his comfort and what is necessary. He says: "I do not suffer if I am quiet; if I do not talk; if the cold air does not strike me."

I have always regarded the operation for the relief of trifacial neuralgia as one of the great triumphs of surgical anatomy. When I first read of Carnochan's excision of the second branch of the fifth pair of nerves, I said: "What a triumph." And I say to-day,

if one patient has been relieved of that terrible disease, malignant tic douloureux, "what a triumph!"

One of the patients of the late Dr. G. C. Blackman, it was said, took one pint of laudanum in twenty-four hours, and still suffered.

I regard the operation with more favor than many of my Fellows; but, as was suggested by my colleague, Dr. Conner, in some of these cases the cause is within—is situated beyond the ganglion higher than the foramen—in these the operation cannot give relief.

Both of the two cases which I have given may be regarded as extreme; but in both relief was had, positive in one, comparative in the other.

My predecessor, Dr. Geo. C. Blackman, had not, I believe, one single case in which the disease did not sooner or later return. Our lamented Fellow, Dr. W. H. Mussy, was more fortunate. In such a study, with such a practice, we should not be hasty; we should not be too quick in reporting success, for in too many instances the pain has returned with all its severity after having been quiet for months; "partial success" could only be written then. Now, after a somewhat liberal experience, I would say, that if six months', aye, if one month's relief could be secured, the operation would be justifiable—it would be doubly so. Every Fellow of the Association who has seen this fearful disease, this unmatched malady, will agree to this proposition; it is a period—it may be short—of exemption from pain in the midst of a life of constant agony.

Dr. Vanderveer. In regard to Dr. Campbell's first inquiry, I have made the same investigation as Dr. Conner, and I have never found any satisfactory reason for the removal of the ganglion The few sensory branches that pass through it do not seem to be any reason for its removal, but yet operators direct that we shall go beyond and remove the ganglion. If the operation is done in a good light, with a trephine large enough to perforate the posterior wall, you can see the ganglion, and remove it without any difficulty. I approached the last two operations without any anxiety at all.

As for the relief afforded, we have no better example of it than the case of Miss B., operated upon in 1875, with no return since. In the other case, where the pain returned after two years, I thought that the removal of this ganglion should have been performed. It seems to me, that, in the patient in whom we had to remove the inferior dental nerve, there was an irritation from the eye. In Dr.

Dawson's case there must have been some central irritation. There are cases, of course, in which we cannot accomplish much, but even in these cases I think the operation is proper.

The President. We may, I think, assume in regard to this operation that the proof of the benefit derived is to be found in the results. If the operation has done good, it is well; if it has not done good, it is not well.

SPLINT FOR THE TREATMENT OF SYNOVITIS OF THE WRIST-JOINT.

By J. M. BARTON, M.D.,
SURGEON TO THE GERMAN HOSPITAL, PHILADELPHIA.

[Read June 2, 1883.]

I DESIRE to exhibit to the Association a splint which I have used for some years in the treatment of synovitis of the wrist-joint. It is light, makes powerful extension, is not conspicuous, and permits some use of the fingers.

The splint is made of wire, and is from eleven to thirteen inches long, and from one-and-a-half to two inches wide. It is placed on the anterior surface of the forearm, reaching from a little below the bend of the elbow to below the lower end of the metacarpal bones. The upper extremity (B) is slightly indented to receive the adhesive plaster used in making counter-extension. On the lower extremity (A) a light wooden-roller is placed over which the extending band plays.

The extension is made by a piece of heavy rubber-elastic band, which is fastened to the upper cross-wire (B) and to the extending band after it passes over the roller (A).

A convenient method of applying the extending band is to sew a heavy kid glove on the hand, cutting off all the fingers of the glove except the middle and ring fingers; these two are split down the back, and retain their attachment only to the palm of the glove; they are passed over the roller and tied by a piece of cord to the rubber-extending band, the upper end of which is

attached to the upper cross-wire at "B". The counter-extension is made by two strips of adhesive plaster, each two inches wide and thirteen inches long. To adjust them, start the first at the wrist-joint, on its radial side, run it up the outer side of the arm, over the upper end (B) of the splint, resting in the depression, and then down the ulnar side of the forearm to the wrist. The second strap is applied in a similar manner; both straps necessarily nearly covering the forearm. A light bandage may be carried from the wrist up to keep the plaster in position; the extending force is determined by the amount of tension of the rubber band, from one-half to one pound of pull being quite sufficient.

EXTERNAL MEDIAN PERINEAL URETHROTOMY FOR CYSTITIS AND FOR THE REMOVAL OF MORBID GROWTHS FROM THE BLADDER.

By R. B. BONTECOU, M.D.,
OF TROY, NEW YORK.

[Read June 2, 1883.]

The operation is a median incision of the perineum, commencing about one inch from the anus and extending about one or one-and-a-half inch toward the scrotum, according to the depth of the perineum and size of the subject, and terminating in the membranous portion of the urethra, which should be laid open just in front of the bladder to the extent of three-quarters to one inch; this is done on a grooved staff while the patient is in the lithotomy position, the bowel having previously been emptied. The operation is simple, almost bloodless, and rapidly and easily executed; the finger may then be admitted to the bladder, and by counter-pressure above the pubes, if the patient is profoundly etherized, every part of the viscus may be made to touch the point of the finger, thereby obtaining most intelligent and thorough examination of the whole interior, and enabling the operator to remove with spoon, forceps, or suture any morbid growth capable of being removed. The operation is especially adapted to cases of irritable bladder from vegetation or polypoid growths, and to cases of chronic cystitis from other causes, that have proved incurable by drugs and hygienic methods. The lateral operation, as for lithotomy, was, in November, 1850, practised by Professor Parker, of New York, for the relief of an obstinate case of cystitis, the result of gonorrhœal inflammation; but this method, much more simple and less dan-

gerous, has not been practised for this class of cases, I believe, until Sir Henry Thompson, of London, in 1882, recommended and practised it on a few cases with gratifying results, and his publications induced me to apply it to two very bad cases that came under my care.

Case I.—F. S., a moulder by occupation, twenty-eight years old, of correct habits, and no specific history, and who was a strong laboring man, until May, 1882, when he took sick with rheumatism, which lasted two months; during this sickness his bladder trouble commenced with scalding of the urine, accompanied by pus and mucus in large quantities; efforts to expel this brought on distressing tenesmus and protrusion of the bowel, and he was obliged to void his urine every half hour night and day, and often the burning in the penis and straining were so unbearable that he was forced to cry aloud. He came under my care in October, 1882. Irrigation of the bladder among numerous other means was tried, but it was so painful to him that he would not submit to it with any regularity, and then only when under the effects of an anodyne injection of one or two teaspoonfuls of laudanum. Hyocyamus and camphor, salicylate of soda, bromide of potassium, buchu, uva ursa and epigea repens, balsam of copaiba, turpentine, and other remedies were given internally; while laudanum injections, opium suppositories, and ergot were used by the rectum; the bladder was washed out whenever he could tolerate it with solutions of borax and carbolic acid, chlorate of potassium, bicarb. sodium, permanganate of potassium, alum and sulph. zinci, with varying degrees of relief. On March 20, 1883, I performed the operation above described, and found two polypoid growths hanging from the upper and anterior surface of the bladder, which I removed with the finger nail and forceps, without producing much hemorrhage; the bladder was thoroughly washed out, and a drainage-tube of large calibre inserted and left in. The tube was retained with some difficulty, owing to the severe contraction of the viscus; but its presence was not painful to the patient, and through this his bladder was two or three times daily irrigated with a solution of borax and carbolic acid, and the mucus and pus escaped freely; the agonizing burning pain in the penis ceased at once, and returned only on a few occasions when the tube had become obstructed, or had slipped down into the perineal wound.

He could sleep a greater part of the night without anodynes, and expressed himself as quite happy compared with his former state. About the middle of April, both lower limbs swelled, and all the joints became painful; these conditions yielded to the internal use of salicylate of sodium. The patient is now well enough to walk some distance daily, and his general health nearly restored to the normal standard. The drainage-tube was kept in use one month, since which time it has not been required, and the bladder empties itself mostly through the natural passage, but occasionally some urine escapes from the sinus, which is not yet entirely healed.

Case II.—I. H. N., moulder by occupation, twenty-six years of age, with no specific history, who for three or four years had been suffering from irritable bladder, took cold about eight months ago, and had a decided aggravation of his trouble; eight weeks ago he was married, soon after which he became much worse, and was obliged to remain in his bed, being unable to walk on account of the painful straining and burning pain in the penis. He was obliged from this time to pass his urine every hour or oftener, day and night, and always accompanied by quantities of mucus and pus. He was admitted to St. Peter's Hospital, at Albany, New York, and remained there ten weeks, and finding no relief returned to his home in West Troy, and came under my care April 4, 1883. Irrigation of his bladder had been practised by himself and attendants since January, 1883; but not regularly, on account of the great pain attending the introduction of the catheter, and often the suffering, which lasted four or five hours after the operation. I did not succeed satisfactorily with the use of internal remedies, and on May 7, 1883, I performed median external perineal urethrotomy, and found just within the neck of the bladder on his left side a cock's-comb shaped vegetation, which I removed with forceps and my finger nails, and inserted a drainage-tube doubled upon itself with two eyes in the inner curves of the turn, and kept in the form of an open loop by a slender brass-wire of sufficient temper to keep the knuckles of the tubes expanded within the bladder; this I found to be self-retaining, and to possess great advantages over the straight tube in retaining its position without tying; the loop was curved downward, so that it would lay in the cul-de-sac, behind the neck, and siphon off the morbid fluids disposed to collect there. Through this double tube the attendant easily irrigated the bladder from a fountain syringe without pain to

the patient; and the relief experienced was, as he expressed it, charming. He was wasted to a great degree by his long suffering, but soon regained his appetite, and enjoyed sleep at night for hours; whereas, formerly, he was every few minutes disturbed, and his suffering from the burning pain and tenesmus was extremely severe, notwithstanding the administration of opiates in large quantities by the rectum. All the pain ceased and has not returned, and he is now dispensing with the tube, and able to sit up and walk about his rooms. Urine still discharges by the sinus, but the mucus and other evidences of inflammation have gone.

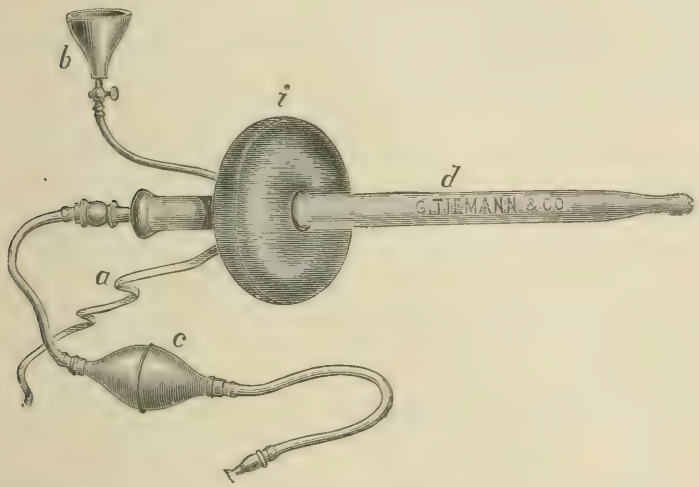
A singular accident occurred in the treatment of this case: a fortnight after the operation a smart hemorrhage occurred, which was controlled by the use of a cold alum solution and subsulph. of iron; which latter ingredient I should advise not to be again used, as the coagulum formed in the bladder was so hard that I was compelled to break it up and remove it piecemeal with the lithotomy forceps.

DESCRIPTION OF A RECTAL OBTURATOR, AND A REPORT UPON ITS USES.

By DAVID PRINCE, M.D.,
OF JACKSONVILLE, ILLINOIS.

[Read June 2, 1883.]

I DESIRE to make a short report upon the uses of a Rectal Obturator in connection with its presentation to the Association.



a. Tape attached to the obturator for withdrawing it after its collapse. The tape or thread may also be passed through the central opening and around over the circumference, and then be tied so as not to be lost.

b. Funnel attached to the pipe, arranged for filling the obturator. This can be replaced by a syringe. In using a syringe the amount of water (measured by the bulb of the syringe) necessary to fill the obturator, should be previously ascertained, in order not to run the risk of hurting the obturator by over-pressure.

c. Syringe for injecting fluids into the rectum.

d. Bougie.

i. The obturator expanded with water. This, in the collapsed state, should be placed just within the anus. A piece of dentist's elastic-rubber dam slipped upon the bougie down to the position of the obturator facilitates its entrance through the anus. The greater approach to a wedge shape enables the obturator to pass the sphincter more easily.

It may be employed to accomplish the following objects :—

1. To enable the alimentary canal to hold two or three gallons of water in cases of obstinate constipation, in cases of obstipation and of strangulation; or, for the speedy and complete washing out of the canal for any condition.

2. To hold in the alimentary canal comparatively large amounts of nutritious fluids in cases in which the introduction of food by the mouth is impracticable.

3. To hold in the large intestine an adequate quantity of alcoholic liquids introduced for anæsthesia in surgical operations, or injuries, or for the alleviation of shock from any cause, or in all conditions in which alcoholic anæsthesia is desirable.

4. To accomplish the speedy dilution of the blood through the absorption of water from the intestinal surfaces in order to stimulate the kidneys to filter out any poisonous constituents of the blood, whether these impurities have been introduced by accident or design, or whether they have been developed in the blood and in glands by the chemical processes incident to disease.

The old masters in medicine sought to accomplish this by very dilute infusions and decoctions, but the concentration of medical substances, by the art of chemistry, has made it necessary to study other means by which to dilute the blood and thus to favor the action of the kidneys.

5. For the reduction of temperature in fever and inflammation by the more ready discharge, through the various emunctories, of irritating material, by the thinning of the blood through the introduction of water.

The use of the stopper obviates the necessity or the indication, for the employment of a long tube, for carrying liquids high up in the colon.

The prevention of the escape of water (or other liquids) by the anus, obliges the liquid to insinuate itself along the easiest road for it to travel, until it escapes by the mouth; the permeability of the ileo-cæcal valve having been proved in special cases by numerous observations of the passage upward of the contents of the colon, and especially by the experiments of Dr. Robert Battey upon the dead subject.

I have made three observations in relation to the question of the permeability of the ileo-cæcal valve upon the dead subject,

In one of the subjects, the large intestine was slowly filled by water, and it was ruptured under the pressure, without the passage of fluid into the small intestine. In the other two subjects the water passed readily into the small intestine, and by moving the intestine, it passed into the stomach—and in one of the subjects it escaped from the mouth. On inspection of the valves it was found that one had the ulcers of typhoid fever in connection with ulcers in the lower portion of the ileum; another was defective from some former disease, and the one which had resisted pressure was perfect.

It is probable that the apparatus will be most employed for the third purpose in the enumeration; namely, for introducing and retaining large quantities of alcoholic liquids in connection with serious surgical operations, in order to prevent the occurrence of shock.

There is one fact which, at first view, is opposed to the use of alcohol to prevent shock, and that is, the depression of temperature which is occasioned by alcohol, while shock is also attended by depression of temperature.

The following observations lend probability to the belief in the capability of alcohol as an anti-shock agent, notwithstanding its influence in depressing the temperature.

Operation for the removal of lymphatics under the jaw, developed subsequently to a plastic operation for the removal of an epithelioma of the lower lip.

11 A. M. Temperature in the axilla 99° F. Preliminary administration of chloroform. Rectal injection of alcohol, 120 c. c. with 600 c. c. of water; the alcohol is supposed to be 85 per cent., which makes 102.05 c. c. of absolute alcohol.

At 12 M. Temperature $97\frac{1}{4}°$, axilla.
" 12.45 P. M. " $97\frac{1}{2}$ "
" 4.15 " " 98 "

Further observations, to show a reaction above the natural grade of temperature, were not taken.

Mrs. S. Operation for the removal of a large ovarian tumor and amputation of a pregnant uterus.

The night before the operation pulse 108; temperature 98° in the mouth.

Preparatory for the operation, 10 grains sulph. quinia the night before, and the same quantity early in the morning; also 30 cubic centimeters of whiskey, repeated in an hour. Operation between 11 and 12. After anæsthesia had been effected by chloroform, an injection into the rectum of 180 c. c. of whiskey, diluted with six times as much water. Ether was afterwards given, as much as was necessary. Immediately before the operation the pulse was 120. The temperature at this time was not taken.

1 P. M. Pulse 120, temperature in axilla 97°.
11 " " 116, " " 99¼.

Next morning 5.45. Pulse 108, respiration 30, temperature 99¾°.

The temperature of the extremities remained good during the operation and afterwards, and there was no depression of the pulse. In the anæsthesia by alcohol, the depression of temperature can only be ascertained by the thermometer.

The capillaries remain full, and the extremities moderately warm. An equilibrium of circulation is maintained, probably through the absence of the reflex influences, which produce shock.

The following observation shows a more decided depression of temperature while the capillary circulation remained unimpaired.

Mrs. C., with malignant enlargement of the lymphatic glands of the axilla.

Temperature in the mouth previous to the operation 98½°. Preparation in the morning by a cathartic of fld. ext. senna 12 c. c., and croton oil ($\frac{1}{16}$ minim.) = .004 c. c. After the action of the cathartic deodorized tincture of opii 2 c. c. with whiskey 60 c. c. was given. Operation at 3 P. M.

Anæsthesia by hydrobromic ether, followed by chloroform, and continued by ether. Alcoholic injection of 85 per cent. alcohol, 120 c. c., with nine times the quantity of water. Axillary temperature:—

4.30	P. M.	Pulse	76,	respiration	24,	temperature	96¾°
5.30	"	"	78,	"	22,	"	95¼
6.45	"	"	78,	"	27,	"	95¼
8.00	"	"	88,	"	30,	"	96½
9.15	"	"	80,	"	28,	"	95½
10.30	"	"	80,	"	28,	"	96¼

The observations were discontinued during the night.

9.00 A. M.	Pulse 120,	respiration 20,	temperature	100°
3.30 P. M.	" 120,	" 24,	"	100½
9.00 "			"	100½

Miss M., aged 50, having a mammary tumor of twenty years' duration, now in the process of gangrene, weighing, after removal, four pounds.

A cathartic was given the day before, and 10 grains sulph. quinia at noon, without dinner.

2.30 P. M. Hydrobromic ether, followed by chloroform, succeeded by sulph. ether. 120 c. c. of 85 per cent. alcohol, combined with 1080 c. c. of water, were injected into the rectum.

Temperature at this time in axilla 99½°. Previous temperature not taken.

3 P. M.	Operation concluded; temperature 100¾°.				
4.00 P. M.	Pulse 112,	respiration 24,	temperature	98¼°
5.00 "			"	96
6.30 "	" 98,	" 24,	"	98
7.30 "	" 80,	" 21,	"	99½
10.00 "			"	100

Next day pulse 104, temperature 100½°; the day after pulse 104, temperature 100½°.

To the touch the axilla was nearly as cool as the hands at the time of the greatest depression of temperature. She slept several hours without any other hypnotic.

From this operation it appears that the greatest depression of temperature is likely to follow any surgical operation of moderate duration. The patient escapes the necessity of hypodermic injection of morphia, and the disturbance of the stomach immediately after an operation.

DISCUSSION.

Dr. T. F. Prewitt, of St. Louis, Missouri.

I have listened with interest to Dr. Prince's paper, and the plan he suggests has struck me as a very ingenious, but not a very con-

venient method. It seems to me that, in my experience, I have been able to fill up the intestine to the full extent of the tolerance of the patient with an ordinary syringe. I have been able to inject until the patient would cry out with pain, and this is as far as we ought to go in any case, because there is danger of rupture of the bowel from over-distension. This has happened and is liable to happen if proper care is not exercised. While this is a very convenient instrument, it occurs to me that it is very likely to invite the use of injections of water and air under some circumstances in which they should not be used; for we may in many cases relieve intussusceptions of the bowel by the injection of water and air without the employment of other means. I have known cases which were relieved by the injection of water; and it is my experience that inflation is infinitely better than water in intussusception. But in other forms of obstruction of the bowel, there are very serious objections to the introduction of either air or water. In the first place, so far as all forms of obstruction are concerned, it is important for us to make if possible a definite diagnosis as to what form of obstruction exists. We may be able to definitely say that it is not intussusception, but we cannot say whether it is volvulus or an internal hernia. Now, when we have an obstruction of some other character, all the distension we can make is likely to increase the difficulty, and especially is this the case, if it is a case of volvulus. Quite a number of years ago I had a case of this kind, when my experience was not as great as it has been since; when I was not able to make a diagnosis between volvulus and other forms of obstruction. Supposing, upon general principles, that it would be a good plan to distend the bowel, I did so. I distended the bowel fully. The patient became pale, and the signs of collapse became evident. The patient died. I found that I had a condition of volvulus. I am confident that I aggravated the condition. I only increased the strangulation.

Dr. PRINCE. I am greatly obliged to the gentleman for noticing the objections to my instrument. As regards over-distension, so far as I know the literature of the subject, there has never been a case of rupture from the injection of water. But there are cases from the injection of air, or from the development of gas by making a soda fountain of the intestines. I, therefore, think that water is better than air, because it is not so elastic. You are aware that in

blowing up a rubber-bag with air it fills up gradually and finally bursts. So in blowing up the intestine, air is more liable to rupture it than water. The pressure with water is not increased.

Then, again, in a case of volvulus, it is practicable to tell where the twist is situated by this method. One who becomes expert on this subject, and knows the usual capacity of the intestines in the dead subject, can tell where the volvulus is situated by the quantity of water that can be introduced. He can also tell whether the obstruction is in the small or large intestine, whether it is high up or low down in the intestine. Now it has been shown, that the ileo-cæcal valve is not a closed one in a state of rest, so that water can be forced in until it comes out at the mouth. The intestinal canal holds about three gallons of water; if you can get in only half a gallon, you can estimate where the obstruction is.

Rectal alimentation is objected to on the ground that there is no digestion. Now it is certain that the higher up we put the fluids, the greater will be the liability of the absorption. The great objection to the use of nutritive fluids is, that only about four ounces are usually introduced at a time. So if the fluids are introduced and held there for a little time, until the spasmodic attempts at dejection pass over, they rise up into the upper part of the intestine, and the albuminates being previously digested artificially, we have the substances converted into an absorbable product.

STRICTURES OF THE ŒSOPHAGUS.

Their Nature and Treatment, with Cases.

By HENRY F. CAMPBELL, M.D.,
OF AUGUSTA, GEORGIA.

[Read June 2, 1883.]

The frequency of injury to the œsophagus, resulting in obstruction and disability of the tube for the entrance of food into the stomach, gives to this class of accidents, and the conditions arising from them, an importance well worthy the careful consideration of the surgeon. Few who have had the unhappiness to witness the gradual, or more or less rapid closure of this important canal; to note day by day the hunger, the thirst, the restlessness, the wild desire, the appealing calls for help and relief, and then the wan cheek, the anxious eye, the rapidly emaciating frame, all harbingers of a sure and agonizing death, can ever forget the scenes, of which they have been the unwilling witness, nor the cries of the victim—most frequently a child—as the painful echoes of a memory they would gladly efface.

In the discussion before this Association of a condition of such widely recognized importance and acknowledged difficulty, I might well be expected to enter into an extended consideration of the observations and experiences of others in both early and more recent times, carefully weighing and comparing their methods and devices for relief, as well as their views as to the nature of the affection, with my own, especially the opinion I hold in regard to the peculiarities presented by œsophageal stricture. This, in a paper like the present, I can only do to a limited extent, on account of the brief time allowed for the read-

ing of communications before the Association. My object is mainly to present my own experience in a number of cases, which, though limited, has perhaps been more extended than that which falls to the lot of most surgeons in their private practice.

Besides two cases in which I was consulted casually, and others treated by my advice without personal examination and after observation, I can at the present time report four cases treated by myself, and under my direction by others after the obstruction had been partially overcome. Of these four cases I am enabled to give the connected history from notes taken at the time, and to give the final result in each, as from the report of those who have had them under observation, and to whose care the continuance of treatment had been confided. It is on account of what appears to me measurably the satisfactory results of the treatment—dilatation, compared to that more recently suggested method of incision, and also to urge the importance of all expedients for the nutrition of the patient during treatment, however protracted and slowly progressed in, that I am induced to relate these cases and make the present brief remarks.

The œsophagus proper is, as is well known, a musculo-membranous canal extending from the termination of the pharyngeal cavity, about opposite the cricoid cartilage, to the cardiac entrance of the stomach, as generally considered opposite the ninth and tenth dorsal vertebræ. In descending, it slightly deviates in the neck to the left side, making it more accessible here to operation, and, it is thought by Cruveilhier, offering some obstruction, on account of the deviation, to the passage of instruments. The stricture is largely, indeed we may say principally, muscular, the fibre being of the striped or voluntary kind, though the functional activity of the canal is only partly voluntary, being largely reflex. This automatic activity is characterized, even in perfectly natural conditions, by a spasmodic quickness and celerity of contraction in the fibres not known in but very few instances to attach to involuntary movements. The existence of the striped fibre and also the quickness of the reflex activity is known to pertain more particularly to the upper portions of

the tube,[1] where it merges into the pharyngeal cavity with its special muscular apparatus, and even more exaggerated reflex responsiveness to excitants. The disposition of the muscular structure of the œsophagus into layers of longitudinal and circular fibres, while it admirably adapts the canal for its function of deglutition, and accelerating the progress of aliments downward to the stomach, also involves the liability to spasm and stricture under suitable conditions of the mucous membrane, which aptitude and liability constitute, as will hereafter be shown, the most important hindrance to the treatment of organic stricture of the œsophagus, as well as to the ingestion of food. This muscular apparatus and this sensitive lining membrane derive, as is well known, their sensitiveness and excitability from an abundant innervation of the pneumogastric nerve—both trunks descending upon the side of the œsophagus and furnishing innumerable sensori-motor filaments to its muscular and mucous coats throughout its entire length. The normal action then, of the œsophagus, being one of alternating constriction and relaxation, it will be easily seen that under abnormal or traumatic irritation, as by the presence of an abrasion or ulcer, or by the existence of an organic stricture, however partial, it would be constantly liable to the occurrence of spasmodic closures in addition, which of themselves might be, for the time being, shorter or longer, of the most obstructive character. I mention this fact in connection with the instrumentalities concerned in their production, because this character of spasmodic closure, superadded to the organic stricture, was a pretty constant feature in all of my own cases, and doubtless is the cause

[1] See a paper by the present writer "On the law governing the distribution of the striped and unstriped muscular fibre," Southern Med. and Surg. Journal, vol. vii. p. 139, March, 1851; also, Transactions of American Med. Association, vol. iv. 1851, p. 468: "Whenever celerity or quickness of action is required in a muscle, under any circumstances, we find the striated or more perfect fibre entering into its composition without any regard whatever to voluntary or involuntary motion." This law, the writer maintains, is of universal application, and explains the three exceptions—the existence of striated fibres in the heart and in portions o the œsophagus and pharynx. These were the exceptions that embarrassed the former law of striped fibre to voluntary and unstriped fibre to involuntary muscles.

of much of the obscurity attaching to the cases of others—and interrupting, apparently, the progress of treatment—when not fully recognized and met by some special method of dilatation to avoid the production of, and to overcome, the spasms.

It has been the habit, or rather the method of reasoning, adopted by most of those who have written upon the subject, in the discussion both of its nature and the treatment to be applied, to have before them as a model and guide strictures of the urethra. This I can but regard as a most fallacious comparison, in which only an imperfect analogy and no real parallel exists upon which to base any safe conclusion as to either pathology or treatment. This misleading mode of reasoning, it is thought, may have induced some surgeons recently to adopt the division of the stricture as preferable to the slower, equally effectual, and far safer procedure of gradual and persistent dilatation.

While I cannot profess myself as giving full acceptance to the doctrine so popular with many of late, that incision, whether external or internal, is imperatively necessary for the relief of urethral organic stricture, I regard these operations as comparatively safe, and often commendable, to shorten and facilitate treatment in many obstinate cases. But such procedures are far different in strictures of the œsophagus—first, on account of the hidden character of the obstruction, inaccessible to direct observation, rendering such cutting operations uncertain and hazardous; and, secondly, because the relations of this canal to the mediastinum, the aorta, and to the anastomosing arterial vessels throughout its entire length, would render an incision of even moderate depth extremely dangerous, and perhaps fatal by a penetration of the wall of the tube; and, lastly, because in my own experience I have as yet found no single case which was not amenable to treatment and improvement, by early gradual and persistent dilatation, with instruments carefully adapted to the peculiarities of each case. Most of the cases are brought to the attention of the surgeon, it is true, only in an advanced stage of the obstruction, and not until the œsophageal disability and defective nutrition have caused an amount of emaciation

and exhaustion which threaten death by starvation. But still these patients can at times pass fluids through the stricture into the stomach. This is the ground of hope for almost every case. If any fluid, however tenuous, and however small in quantity, can pass, it may be regarded as a matter of certainty that some form of instrument of carefully selected material can, by patient, careful, and persistent effort, be made to traverse the still remaining area, or what may be regarded the gradually closing canal, and thus secure to the sufferer the prime condition upon which relief by dilatation is to be accomplished, viz., the possible perviousness of the stricture. Any case outside of malignancy in which this can be done is susceptible of relief, if not of permanent cure, by dilatation.

The causes of œsophageal stricture are known to be various. Malignant tumors and malignant degeneration of the walls of the canal, though in their results the most surely fatal of all obstructions, are not, I think, properly classed among true strictures of the œsophagus, as the term stricture would be applied to other canals of the human body.

As my own experience has been almost entirely restricted to cases arising from a single cause, and that cause of a nature capable of producing œsophageal stricture in its most typical form, we have no need to be concerned about the subject of classification in the present paper.

The rapidly increasing manufacture and almost universal purchase and use in the domestic life of certain classes of the salts of potash and soda—"concentrated lye"—in washing, scouring, and other processes of cleansing, render these active caustic preparations more frequently than all other causes combined, the terrible agents by which for some years past, and we greatly fear with increasing frequency for some years to come, this fearful and often fatal condition of the gullet is produced. Concentrated lye and concentrated potash are so constantly reported by rumor and in the daily journals, as well as by medical men, as the cause of death to children by the accidental or unwilling swallowing of the solutions, that the reflective and humane would gladly advocate some legislative enactment which would

place them among the poisons too dangerous to be entrusted to the hands of the people, or to be used in their present forms as a common article of domestic life.

The four following cases will serve to illustrate the frequency of these accidents as produced by this class of agents as they also comprehend the principles and method of our treatment:—

Case I.—Polly, a colored woman, aged about thirty-eight years, a servant of Mr. P. Fleming, of Augusta, Ga., was, in the winter of 1856, the subject of gastric neuralgia, for which I had prescribed a sedative mixture in which chloric ether and bicarb. soda entered as ingredients; being suddenly seized with one of these gastric attacks she called a younger servant to pour out a wineglassful of the medicine from a bottle. On drinking the contents of the glass she was seized with the most distressing burning in the fauces, and gullet, and epigastrium, which caused her to vomit with great distress the contents of the stomach, and with it probably most of the caustic potash which she had thus taken by mistake.

I was called immediately to the patient, being only a short distance from the house. I caused her to take at once nearly half a tumblerful of olive oil taken from the table cruet. This was to saponify the alkali, and arrest the process of chemical injury to the mucous membrane of fauces and stomach. This was vomited in a short time only slightly changed in character and appearance. The dose was repeated, and about a half glassful of the sweet oil was retained. Nausea and vomiting with burning pain in throat and epigastrium continued for many hours, but was relieved by morphine. Blood and bloody mucus soon began to appear in the matter vomited and expectorated. This patient began at once or within a few days to complain of great difficulty and "spasm in the throat" in all attempts at deglutition; even water was swallowed with great difficulty and pain, though, as she expressed it, it gave her delightful relief from the craving and burning thirst which all the time tormented her.

Deglutition, which was from the first painful and obstructed, in less than two weeks began to be, for hours together, impracticable, and then rather suddenly and unexpectedly she would find some of the fluid to pass into the stomach, but always slowly. The swallowing of solids and semi-fluids had been impossible from the time

of the accident. Finding this woman rapidly becoming weak and emaciated, and not then having the implicit reliance upon rectal alimentation which I now entertain, I determined to begin the process of gradual dilatation at once as an imperative necessity. There were evidently abrasions and unhealed excoriations somewhere in the canal, as was shown by the sanguinolent discharges coming up with the fluids returned on her attempts at drinking. Starvation was staring her in the face, and the face returned the stare with a fixed and abiding expression of hunger, anxiety, and distress, which looked more like the glare of insanity than that of any bodily distress. On attempting to explore the gullet with an ordinary œsophageal bougie of moderate size I found the instrument produced much pain, and was obstructed in the passage a little below the cricoid cartilage. There being much spasmodic action, a No. 10, and then a No. 5 gum-elastic urethral bougie was tried, when the latter passed with some difficulty. This was allowed to remain for a short time, when the No. 10 passed without much more difficulty.

I now passed a conical flexible gum-elastic French catheter, No. 10, beyond the stricture for the purpose of introducing water into the stomach: A glass of water and a glass of milk were thus injected to the great relief of the sufferer.

As the case progressed towards recovery, it was found that the improvement was often irregular, that is, the size of the bougie could not always be increased from one dilatation to another, but at the beginning of each sitting a smaller size than the instrument last used had to be applied before any advance could be made by the introduction of a larger dilator. This apparently discouraging circumstance in the treatment must certainly have been due to spasmodic action of the circular fibres, and not to any real narrowing of the space that had been gained, for it was observed mostly during the earlier periods of the treatment while yet the cicatrices were tender and irritable, and while, perhaps, there may yet have remained some unhealed abrasions or ulcerations at the point of injury. This spasmodic irritability continued many weeks, entirely preventing deglutition, even of fluids, and during which time the daily injection of one quart of milk through a catheter into her stomach was her only sustenance. This spasmodic closure of the gullet would occasionally take place some months after she had been able to swallow both fluids and solids, and when the ordinary bulbous

tube of the stomach-pump could be passed with only spasmodic obstruction for the introduction of water or milk.

After having ceased treatment in this case, she having long since recovered completely the power of swallowing solids, she was taken to the upper part of the State by her employers. While there she was suddenly seized with complete disability to swallow either fluids or solids. The neighboring physician was called in, and all his efforts being unavailing, he proposed that a *mesmerist* who was in the village should see her, saying she must die unless he could relieve her. On arriving and being informed of the nature of the case, he said he would try to relieve her, and called for a glass of water and a piece of corn bread, after which he "thoroughly mesmerized her," and handing her the water, he commanded her to drink it, which she did without apparent difficulty. He then desired her to eat the bread; she hesitated, then rather demurred, and finally ate the whole of it. After having lost sight of this patient for many years, I was sent for in haste. the message being that "Polly's old disease had returned upon her." Being unable to visit her at the moment, I sent her one of the ordinary œsophageal bougies she had formerly used, but the choking had ceased, and she did not find it necessary to resume the self-treatment which had been pursued for a long time after leaving my immediate care.

These suddenly occurring and persistent obstructions to deglutition, relieved in the one case at the behest of a mesmerist, and in the other ceasing spontaneously, cannot fail to be recognized as being of a reflex or spasmodic character, affecting the circular fibres; no organic constriction could either occur or be removed so suddenly by any such influences.

Case II.—Child of J. J. Anderson, Williston, S. C., aged about eighteen months, had stricture of the œsophagus caused by drinking "concentrated lye" some months previously. At the time of the first examination, August 16, 1881, the child was greatly emaciated, fretful, and crying for water and food. On making attempts at deglutition, some of the food, cake and bread, seemed to enter the upper portion of the gullet, but would be returned in a short time with the water or milk taken, neither solids nor fluids appearing to pass the stricture. The mother reported that the child had been unable to swallow any water or food for four or five days—which period of disability she said was not uncommon, as he had had

several times before prolonged spells of "stoppage," at the end of which he would be able to get down water and milk, with difficulty, for several days at a time. She reported one of these spasmodic closures as lasting nearly nine days, during which the child had suffered great distress and nearly died of hunger. The extreme emaciation and exhaustion of the child, its present and constant distress from prolonged starvation, rendered it important to proceed at once in our attempts at nutrition.

The child being held firmly, and its mouth kept open, assisted by Dr. J. S. Coleman, a Fellow of this Society, a No. 6 bougie was passed, for the purpose of exploration, down to the stricture, which appeared to be a short distance below the cricoid cartilage. The instrument was arrested at this point, and after some cautious and delicate attempts to push it further it was removed. It was covered with thick mucus, mixed with milk and softened bread crumbs, which the child had been attempting to swallow. A No. 8 gum-elastic bougie, with a soft, flexible, and conical point, was now introduced, for the purpose of passing or entering the stricture if possible. The instrument being well lubricated with vasaline, was passed over the finger of the left hand holding down the tongue. No force was or could be used with this soft and very flexible instrument. When it became arrested at the point of the stricture, as the other one had been, it was slightly withdrawn and again gently propelled. This manœuvre being repeated several times, the point soon entered and passed the contraction, when it was with some obstructive compression pushed on into the stomach. On the removal of the bougie, the mother asked if she might give the baby some water, because, she said, "I see by his countenance he can swallow." She further stated that, now being relieved, he would be able to take water and milk for some days, "until he had another stoppage." First water and then milk was given to the child, several wine-glasses of which latter it was allowed to take. It swallowed slowly, but without much apparent spasm. The mother stated it would gradually be able to drink more freely.

As the great and most pressing object was the present nutrition of the child, the mother was directed to supply it cautiously with milk and beef-tea or meat juice. We also directed that these articles should be supplemented by nutritious injections, if a sufficient quantity of fluid nutriment could not be taken naturally. No time was appointed for a second dilatation, but the parents were directed to

bring the child whenever there was a recurrence of the spasm and disability. The child's vitality was so low, that I was not willing to interfere with its gullet as long as it could take nourishment of any kind for the improvement of its health. In about a week the patient was again presented, the same proceeding of dilatation with a flexible, conical gum-elastic bougie, when the child was again relieved.

We found in the several repetitions made, that the instruments, though increased in size, passed more readily each time, and that the intervals between the spasmodic closures were longer, while the child increased in flesh and plumpness.

The season being unfavorable for a delicate child to remain in the city, he was sent home to the care of Dr. John Smith, of Blackville, S. C., who had referred the case to me. The following extract from Mr. Anderson's letter, dated November 19, 1881, four months after beginning of treatment, will show the favorable progress up to that time, both as to improvement in deglutition and general health:—

"The baby is no better in regard to eating any solid food, but he can drink milk. He does not have those long spells of closure of the gullet, as when he was with you; and when he does have it, it lasts not more than an hour or two, or not more than half a day the longest. Dr. Smith has attended him ever since he left you, and has used the bougie a great many times. Sometimes when his throat is closed it gives relief, at another time it seems to do no good. He has increased surprisingly in flesh and strength, and can talk as plain as any child. He has not forgotten you, as small as he is. We can't keep him in church, or in any crowd of people. As soon as he gets there he begins to cry and say, 'The Doctor bother me!'

(Signed) J. J. ANDERSON."

The above encouraging and measurably satisfactory account of the case reasonably gave hope of progressive improvement and of a possible ultimate recovery. In preparing our notes, however, for the present report, the information obtained nearly eighteen months after from Dr. W. W. Smith shows that the case afterwards resumed its former unfavorable characteristics.

WILLISTON, S. C., May 10, 1883.

DR. H. F. CAMPBELL.

My dear Doctor: I received your letter the other day inquiring as to the treatment of James Anderson's child. I have been wait-

ing to see Dr. John M. Smith to get the history of the case, but as I have not had an opportunity to see him, and not believing that he could give you anything interesting or profitable, I will just give you what I know of the case. From the time of drinking the concentrated lye, it lost the power of deglutition to a great extent, from which it never recovered. He used various remedies without any good effect. He was never able to effect anything by dilatation. It lived for some time on sugar alone, just as it would dissolve in the mouth, and finally died of inanition.

Yours truly,
W. H. SMITH.

By a comparison of the above two communications, it is evident that the child had been for a time successfully treated by Dr. John Smith with dilatation, as the father reports his frequent and diligent application of the bougies, and the most decided improvement of the child in deglutition and in its nutrition, and also the greatly diminished frequency and persistence of the attacks of spasmodic disability of the gullet. In cases of this most deplorable class there are many things which interfere with the progressive and ultimately successful treatment by dilatation. The alarm of the little patient at the bare idea of the instrument ("Doctor bother me") expresses it fully. The disinclination of the parents to distress the child, so long as it can swallow any food whatever; the engrossing and crowding-out employments of the physician, often a village practitioner with a clientel extending over several counties of the State, all combined to render it probable that progressive dilatation and an overcoming of the spasmodic tendency will not be accomplished even after having been fairly inaugurated; but that, on the other hand, from time to time, the systematic course will be interrupted. The amount of food ingested is so gradually diminished, and the emaciation so imperceptibly advanced, that by the time the parents in alarm compel the attention of the doctor, recontraction to the original degree has taken place, and all that has been gained, though not impossible of recovery, has been, under the circumstances, forever lost, and the child doomed to death by inanition, for want of a longer continued and more systematic treatment.

The note from Dr. Simpson Russ, at the end of our next case, cordially acknowledges his failure to perfect a cure on account of the hindrances to which we have just referred.

Case III.—Daisy Crouch, aged about two years, brought by her parents early in 1882, with disability to swallow even liquid food except at long intervals and with great difficulty. The child was pale and languid, unable to stand, and in a condition of extreme emaciation, with an expression of anxiety and pain. Regarding it from its appearance to be a case of entero-colitis, or cholera infantum, I inquired as to the frequency of its daily evacuations, when the mother told me that the child scarcely ever had an evacuation, as it never ate anything on account of its throat, which had been injured by drinking concentrated lye. The exact date of the accident is not noted. The child was "playing round" at the spring where the grandmother was washing clothes. Unobserved, she suddenly took from the wash-bench a tin can of the concentrated lye and drank some of it. The effect was described as dreadful, the child crying and vomiting, and bringing up mucus and blood, while everybody thought it would die before morning. It finally got better, but was hardly able to swallow anything, and began "to perish away," when it was taken to Dr. Russ, of Graniteville, who, on account of its condition of impending starvation, brought it to Augusta for consultation. In nearly all these cases an examination is made with much opposition on the part of the little patient, and not without the exercise of a good deal of tact and persuasion seconded by more or less force. The child had attempted to drink water and milk, but from the quantity and instantaneousness of its rejection, it was evident that very little or none had passed into the stomach. Though greatly alarmed at the appearance of instruments, this languid little patient made no vigorous resistance to the introduction of the bougies, and a No. 6 flexible conical bougie was, by delicate effort, passed beyond the stricture. This was soon followed by a No. 8, and then a No. 10. No pain seemed to attend their introduction, for after this the child was able to drink, first water and then milk. Being fully convinced of the value of dilatation in this case, as indicated by our first application, a selection of the proper flexible pointed gum elastic bougies, from No. 8 to No. 12, was advised, and the patient returned to Graniteville to the care of Dr. Russ. In case of failure of deglutition, rectal alimentation was advised, either as the sole and reliable means of nutrition, or as supplementary to the small and precarious amount of food that could be carried past the stricture into the stomach.

Desiring to know the subsequent history and final result of the above case before making our report, I received the following brief and candid note from Dr. Russ:—

GRANITEVILLE, May 15, 1883.

Dear Sir: Your card of inquiry in regard to little Daisy Crouch was received a few days ago, but I am not able to make any report, because she moved from here some weeks after you saw her, slightly improved. She returned a short time ago to this place, where her father is again employed; she is as *fat as a pig*, and seems to be perfectly well, but is not able to swallow solid food without great difficulty. I got tired of working with her at that time, it being so difficult to introduce the instrument on account of her aversion and fear of being hurt. I am satisfied that she could have been entirely relieved if the treatment could have been kept up. I am sorry that I cannot help you out much in the case, and especially as I am, to a certain degree, responsible for the failure.

Yours respectfully, etc.,
SIMPSON RUSS.

From the above very imperfect history it will readily be perceived that treatment for only a brief period, together probably with the healing of the denuded surfaces and gradual subsidence of the reflex irritability, resulted in rescuing the little sufferer from impending starvation, and restored to the gullet a competent capacity for deglutition of fluid nutriment, and a slight capability to digest some solid or semi-solid food. Without the judicious use of dilatation at the time of greatest need, it is the belief of the writer that the reflex excitability, and the spasmodic closure of the canal, would have so entirely occluded the channel and shut out nutrition as to insure the death of the child; even though, as we have seen, the organic narrowing could not have been of itself entirely obstructive. I believe that a resumption and continued use of systematic dilatation would still further improve, or entirely relieve the child of its disability. The following case, though one of greater severity, and attended probably by more serious injury to the structure of the œsophagus, will illustrate the advantage of a systematic, regular, and prolonged treatment by dilatation:—

CASE IV.—T. L. Chance, aged nineteen months, emaciation extreme and every indication of threatened dissolution from inanition.

Stricture of the gullet, located apparently a short distance below the cricoid cartilage, had resulted from the accidental taking of a solution of concentrated lye. The child had for some time been unable to swallow any solid food, and very little water or milk. Of the four cases here reported, this one was by far the most distressing to contemplate, and offered apparently the least hope of benefit from treatment.

A near relation of the family, and one deeply interested in the child, said to me: "Doctor, we all felt certain that the child would die, and, as for myself, I often secretly wished that it could die at once, to end its terrible and helpless suffering."

By the cautious introduction of the ordinary blunt-end gum-elastic flexible bougie, No. 8 in size, we ascertained the situation of the obstruction, but made no attempt to pass the stricture. A No. 6 gum-elastic bougie, with a very flexible, soft, and attenuated end, was now carried into the gullet, and delicately manipulated up and down, at the point of constriction, till it had evidently entered the narrow opening. It was propelled on toward the stomach without meeting any perceptible hindrance, when it was removed, and a No. 8 was passed with little or no more difficulty than the first. For the succeeding three days no very decided advance was accomplished, and the child was allowed to return home with its parents, after having furnished them with graduated sizes of the proper instruments by which they were instructed to cautiously continue the dilatation. Rectal nutriment, with meat broths and milk, was advised as a supplementary means of supplying nutriment. Observing in the parents of this child a clear intelligence and full comprehension of what was required, with aptitude and firmness to carry out the treatment, I thought best to advise that the dilatation be performed by them, as equally safe, less alarming to the patient, and securing more perfect regularity of application than could be expected from any medical attendant. With this view the dilatation was repeatedly done in their presence, and every step in the process carefully explained. The case, however, was remanded to the general care and observation of the family physician. Instructions to return to Augusta for further examination and advice when necessary was also given. The history of the foregoing case is perfected in the letter of Mr. Chance (father of the patient) in reply to questions sent him during the preparation of the present report, about six years after the accident.

LAWTONVILLE, GA., May 25, 1883.

DR. HENRY F. CAMPBELL, Augusta, Ga.

Dear Sir: Yours of the 21st at hand. I will endeavor to answer your questions as best I can.

1st. Name of child, Thomas Lanier Chance.

2d. Age at time of accident, sixteen months.

3d. Date of accident, April 1, 1877.

4th. Time elapsed between injury and beginning of treatment by Dr. Campbell, about three months.

5th. The difficulty of swallowing food began about a month after the accident.

6th. The child was emaciated to a very great extent; was nothing but skin and bone.

7th. There was a discharge of blood at times when I used the probang or instrument down the throat, to open the stricture; nothing like the lining of the gullet was discharged, but there was a very thick mucus.

8th. The first time I brought the child to you, I spent three days in Augusta; the next and last time only one day.

9th. We dilated the throat for about one year, once or twice every week.

10th. The child's present condition is very good. Has not been troubled in swallowing for about three years; only that while eating at times it will get choked, but for a short time. On taking a swallow of water or milk it will get all right.

11th. He is very healthy and fleshy at this time. I consider him all right as far as I know.

12th. We used injections of milk for about one year. We fed him only on very light crackers after he got so that he could swallow anything.

Please give me your opinion as to his future condition; as to what you think of the stricture ever closing again. Hoping you may be able to gain the required information from this, I am very respectfully yours,

(Signed) R. C. CHANCE.

I have been thus particular in securing and presenting, sometimes perhaps tediously, all the facts and minute details pertaining to the progress and the ultimate result of the four cases of œsophageal stricture from chemical injury, in order that the

beneficial results of treatment by dilatation may be illustrated. By a careful review of these facts, I think it will be readily recognized that just in proportion to the regularity and prolonged application of the dilatation, till the capacity of swallowing solid food is attained, will the benefit be progressive and the ultimate result satisfactory. Whenever, by the use of natural deglutition, the solid alimentary bolus can pass the stricture, the improvement is apt to be accelerated, for each such passage of the bolus is attended in a certain degree with the same practically beneficial result of the bougie dilatation, and from that time on the reflex excitability gradually diminishes, until an occasional choking, at longer and longer intervals, is the only trace left of the extreme and impending fatal spasmodic susceptibilities of the injured gullet. Timely and judiciously applied dilatation, if continued even for a brief period, will often rescue the patient from impending death by starvation, and secure a food-way fully adequate for the purpose of nutrition. In a communication from Dr. B. F. Wyman, of Aiken, Georgia, we have illustrated the great value of prompt measures, and the good results of systematic dilatation even with instruments but imperfectly adapted to the purpose. We condense the following summary from Dr. Wyman's letter.

Case V.—Robert Brown, colored child, aged six years, had accidentally swallowed concentrated lye about one month before. His mother said his mouth had become very much swollen immediately after drinking the lye, and that subsequently it became very raw, and that he had been able to eat scarcely anything since, first on account of the soreness as she supposed, but during the last two weeks the mouth had gotten well, and yet he could not eat any solid food, and even fluids were swallowed with difficulty, and as soon as he had swallowed (even a little milk) that it would be vomited up again. On this account she thought his stomach must be still sore. Upon examining the child I found it exceedingly emaciated; in fact almost a skeleton. I requested that some milk be brought and the child allowed to drink it. He seized the cup with avidity, attempted to drink it, but would strangle, and the milk, or some portion of it, would regurgitate through the nostrils. After

drinking about half a teacup of the milk, the greater portion being wasted in the attempt to swallow, the child almost immediately thrust his forefinger into his mouth as if to gag himself (in order to induce emesis), and the milk was vomited up. I at once suspected there was stricture of the œsophagus near the opening into the pharynx, and probably another stricture at its termination at the cardiac orifice; either that, or there might be still an ulcer remaining at the latter point. On introducing my forefinger (which was unusually long and thin) into the pharynx I found almost complete absence of the aperture of the œsophagus, caused by cicatricial contraction. I proceeded slowly to overcome the stricture by gently inserting the first phalanx of my finger into the orifice; which, after some trouble, I succeeded in accomplishing. I then, by using a small probang, succeeded in clearing the passage to the cardiac orifice of the stomach. This procedure was kept up for about a month, the patient being fed on milk and lime-water in a tablespoonful dose every two hours during the day for several days; after that some solid food was gradually allowed. This patient made a good recovery, and suffered no further inconvenience as long as I kept him in sight.

Case VI.—Martha Knight, colored girl, aged three-and-a-half years, brought to my office for treatment, the parents of the child stating that some time previously (I forget how long) the child had swallowed some concentrated lye, and since then she had been unable to swallow any solid food. I found the condition almost the same as in the above-reported case—great emaciation and considerable general debility. Upon examination I found some stricture of the aperture, though not nearly as much as in the other case. I found very little difficulty in overcoming it so as to introduce the end of my finger, and by cutting down a sponge probang as small and pointed as possible I succeeded in reaching the cardiac orifice. The treatment was the same, and the patient made a good recovery. This child had no vomiting, simply regurgitations of food and liquids.

Dr. Wyman remarks further: "In connection with this subject I will state that I have seen several fatal cases resulting from the swallowing of concentrated lye. In all these cases death was produced by suffocation; the caustic lye causing swelling of the epiglottis and upper portion of the trachea, thereby rapidly cutting off the supply of air to the lungs from œdema of the glottis.

The Frequency of Chemical Injury to the Œsophagus. Fatal Cases, and others without Treatment.

From the above, and from cases that have transpired within the knowledge of the writer, it may be justly inferred, that the cases of chemical injury of the gullet, especially by concentrated lye, which survive to result in stricture, and apply for treatment, constitute but a *moiety*, while perhaps a majority die from the immediate effects of the caustic, or before any curable treatment has been made available.

Case VII.—Child of Mr. Stephen Bush, of Edgefield County, S. C., one mile from Augusta. Concentrated lye was swallowed by the child. Fearful excoriation of the mouth, lips, tongue, and fauces was produced. Œdematous swelling soon followed. Dyspnœa supervened, with closure of the air-passage, and the child died in twenty-four hours, from œdema of the glottis. The case was observed and reported to the writer by Prof. N. C. Eve, of Augusta.

Case VIII.—Austin, aged eighteen months, child of Mr. T. A. Boyle, of Augusta, while in Nashville drank from a tin-can concentrated lye used by a servant in scouring the floor. The child crawled to the can and drank a little before it could be stopped. Olive oil was given. Great excoriation and inflammation of the mouth and fauces resulted, and œdema of the glottis supervened. Prof. Duncan Eve, of Nashville, was called; but treatment was unavailing, and the child died in less than twenty-four hours.

Case IX.—Walter, aged six years, son of T. W. Boyle, brother of T. A. Boyle. I was consulted in behalf of this child—a healthy boy of six years. When seventeen months old he had found a can of concentrated lye in a closet He "took some of the lye in his mouth, when it was taken away by his mother." Great inflammation of the mouth and throat followed, but by application of oil the child recovered without stricture. I found in the case at this time some slight abnormality in the muscular apparatus of deglutition, and occasional spasmodic movements. These symptoms were doubtless the result of the injury during infancy. As they were thought to be less marked than formerly, no treatment was instituted.

This case is mentioned in connection with the preceding one, to show how liable to accident with these dangerous poisons are children; a second case occurring in the same family, despite the warning and the alarm produced by the first.

Case X.—Colored child, aged two years, drank by mistake "concentrated lye," used by the mother for cleansing pots. Much swelling of the mouth and throat was caused, and the child was in great agony. Mr. J. W. Panknin, a prominent druggist of Augusta, who reports the case, prescribed olive oil, of which he furnished half a pint, to be taken freely. Subsequent history not known.

Case XI.—Clarence Gordon, colored child, aged two-and-a-half years, residence Augusta, Ga., drank a solution of concentrated lye. Mouth, tongue, and throat were severely burned; could not swallow for many days. Then took fluids with difficulty. The child gradually "perished away," and died about a year after the accident, no dilating treatment having been used in the case.

Case XII.—Theodore George Tompkins, colored, aged thirteen months, drank from a tin can containing a solution of concentrated lye used by the mother in scouring the floor. Mouth, chin, and throat, as far as could be seen, "badly scalded." The child could only swallow water. A physician was called. Lead lotion was used to the mouth. The child became extremely emaciated, and died in five weeks. There was no attempt at dilatation.

Case XIII.—Theodore Henry, brother of the last case, aged four years. After the death of the first child, concentrated lye was no longer used in the family. "Washing soda" was substituted. The child drank some of a strong solution of the soda. Much injury to the mouth and throat was caused, and the child pined away, and died two or three months after.

Case XIV.—Furnished by Dr. R. H. Baker, Augusta. A colored child, aged two years, swallowed concentrated lye. Injury to mouth and fauces. Stricture and inability to swallow followed. Case brought to Dr. Baker in extreme emaciation, and apparently too far gone for treatment, and was soon after reported by the mother as having died, six months after time of injury.

Case XV.—Furnished by Dr. G. W. Mulligan, of Washington, Ga. Lucy Williams, four-and-a-half years old. This child had been in the habit of drinking water from a gourd. The mother had made a solution of the "White Rock" potash in a gourd, and left it, June 27, 1874, on a bench. The child, thinking it was water, drank perhaps an ounce of the very strong solution. The mouth, tongue, and fauces found of a deep red color, with here and there ashen spots. Complains of great pain in gastric region. Occasional attempts at vomiting. The usual remedies used. Difficulty of breathing added, and on the 29th the child died, asphyxiated, caused, I suppose, from œdema of the glottis.

Case XVI.—March 4, 1882, Tommy, aged two years, child of Mr. M. J. O'Conner, swallowed a solution of concentrated lye, used by a servant in scouring the floor, lived fifteen months after taking it, dying May 20, 1883. Though suffering dreadfully in the earlier stages, no stricture was discovered until November, when milk sucked from a bottle was constantly regurgitated, and a stricture was discovered. For five, seven, and even ten days at a time, after its first discovery, the child would be unable to swallow fluids, and nourishment was administered entirely by enemata of milk, when suddenly the power of swallowing would be regained, and for three weeks or more the child would be nourished again in the natural way. The mother, Mrs. O'Conner, from whom this account was obtained, reports that dilatation was tried by Dr. Edward Geddings without success. The spasmodic character of the obstruction in this case will be readily recognized from its similarity to Case II. (Anderson's child), which has already been commented upon in this paper.

C. T., a girl five years old, with childish curiosity, put to her lips a solution of potash, kept in a bottle in the kitchen for cleansing purposes. The contact of the caustic with the mouth being painful, it was evidently withdrawn, and she escaped without serious injury.

Archie B., son of Dr. S. C. Eve, aged four years, observing, as he supposed, a can of condensed milk, of which he was very fond, climbed to an upper shelf in a pantry, and thrust his tongue into the whitish semi-fluid mass it contained. His tongue was severely excoriated, but he experienced no further damage from the enterprise.

To this last collection of cases might be added two or more others occurring within the same limits of locality, concerning which, however, our knowledge is not accurate, except as relating to the fact that serious, and in one case fatal, injury resulted from the accidental drinking of concentrated lye. It may here reasonably be asked, why report a number of inconclusive cases, in which neither the treatment, nor its results can be recorded? To the implied rebuke we patiently answer: That though indeed the primary, and perhaps most obvious object of the present paper, is to study carefully the nature and treatment of strictures of the œsophagus, resulting from chemical injury, in the light of my own experience and observation, yet as an object scarcely less important, and far more widely beneficial is that of showing the fearful frequency of such accidents, and the fatality resulting from them, as will awaken an interest in the restriction of the sale and careless use of the caustics causing such direful results. Early in the present discussion has been foreshadowed our proposition that such humane and wholesome object should be attained through legislative enactments, either in the State or by the general government. This is an object appealing far more to the sanitarian than to the surgeon, though it is necessarily through the surgeon and the general practitioner that the deplorable history and frightful carnage—burning of the innocents—by this insatiate Moloch of the household must come.

The Sale of Poisons.

In nearly all of the States, and throughout the dominion of enlightened people, if not in every one of them, laws have long since been enacted placing under the most careful and imperative restraint the sale of a considerable class of medicines, recognized as poisons by the druggist; by special provision or by their humane caution, this intelligent class of dealers carefully label as "poisons!" while often the addition of the death's-head and cross-bones proclaim to the unlettered and unwary the lethal nature of the drug they are about to handle. But how is it with

the sale and distribution of these potent and destructive chemicals, to the ravages of which we have had so often to refer?

The Potash Fiend, "Continental Concentrated Lye," the Potash Ball, et id omne genus.

For many years past there has been growing up in this country, and possibly in England and other European nations, a trade of the most active, and probably to those engaged in it, of the most profitable kind, in cleansing agents; the intent and functions of which seem to be supplemental and additional to the soap trade.

This commerce deals almost exclusively with alkalies, and had extended to the Southern States, as many older citizens will remember, when nearly fifty years ago common carbonate of soda—"lump soda," as it was then called—began to be used by the washerwomen, scourers, and paint washers as a quick and thorough remover of grease and dirt from soiled clothes, floors, and painted walls. This alkali was dissolved in varying proportions—generally a lump "the size of a piece of chalk," in a bucketful or washtubful of water, was the indefinite formula which guided the intelligence or the stupidity of those who used it.

The amount of labor in washing was lessened, and probably money was saved in the expenditure for soap; but this reckless use of the lye was early discovered to injure the texture of the clothing, and it soon became unpopular with housekeepers, so that no laundress could obtain employment who could be convicted of "using soda in the water." Many phases of the same labor-saving devices, all claiming a more excellent way and "not to rot the clothes," engaged attention and sometimes confidence for awhile, were tried, found wanting, and discarded by all intelligent housekeepers and honest washerwomen, on account of the injurious effects of all of them upon the clothing so treated.

It is unnecessary, as it would be inappropriate, here to follow a history of these various devices, for they cover a period of over

half a century. At best, it was but a contest, in which laziness and dishonesty on the one hand, were opposed on the other by watchful intelligence and enlightened economy. For once and for a while the right did seem to triumph, but in later days the evil in another form again broke out, and has gained a more general and destructive spread than ever before; and, horrible to tell, with ghastly addition to its triumphs—the destruction of human life, swelling the bills of infant and child mortality, I do not hesitate to say, by hundreds every year in this country alone. At the present day there are manufactured and sold in this country two or three articles of the nature referred to, one of which at least has gained a fearful prominence in the sad catastrophies which have attended its careless use, or, as would appear, its *misuse*. Concentrated lye is an article, the exact process of preparation of which is not known, except, we suppose, to the manufacturers. It is sold in painted tin cans, covered by a white label. On the labels of that which appears to be the most popular brand, is printed, after the manner and intent of a trade-mark, "Continental Concentrated Lye." Directions are given in French, German, and English, for the making of soap by the use of the contents of the can in gallons of water with pounds of fatty materials. The large amount of water and grease it is capable of saponifying, would alone indicate to the scientific its concentrated strength as an alkali, and consequently its destructive energy as a caustic. The report of cases in the present paper, as well as the invariable results of all accidental swallowing of it, fully prove how direful and destructive are its effects; and yet on no part of the label or can, nor on any wrapper inclosing this terrible poison, is there the least intimation that danger or death, or injury of any kind, is to be even suspected! This can, with its white cover, illuminated label, and finely printed directions, and closely resembling in form and size a can of condensed milk, or corned meat, or choice comestible, is sold as freely and unrestrainedly, with no more questions asked and no more cautions given, than in the sale of the most innocent and harmless article of food and luxury.

In ninety cases out of a hundred its professed and legitimate

intent of soap-making is never carried out by the purchasers, but in various ways it is most ignorantly, carelessly, and dangerously handled. The can is left open, strong solutions are made of portions of its contents for various purposes of cleaning, and it is not surprising that the unwary and the innocent should fall victims to their ignorance of danger in the cup, or to their infantile curiosity.

"*The star ball potash*," unequalled for purity and strength, is another preparation rivalling in commerce and domestic use the concentrated lye. It consists of a mass of potash inclosed in a coating of resin, like the concentrated lye. Though its professed object is the making of soap, it is much more largely used as a cleanser. After being made into a solution of various strengths, it is perhaps less liable to entrap and deceive than the lye, but is handled with equal freedom and carelessness in families, and is not the less capable of destroying life than the one we have shown to have been so fatal in the Southern States.

In the present paper we have depended upon the dozen or more cases collected, most of them within a circle of not over twenty miles in extent, for what is supposed to be adequate illustration of the frequency of the accident arising from the unrestricted sale and careless handling of these caustic preparations. Had the usual and more thorough plan been adopted of sending interrogatives to the members of the profession to elicit individual observations and experience on the subject, it is believed the record would be a most frightful one, in which cases of death or injury, instead of a dozen, could be counted by hundreds throughout the South, and perhaps in all sections of this country, nearly all arising from the same cause. What better could be expected? These caustics are sold in every grocery store in every city, village, hamlet, and cross-road of the country, without caution or even hint of their destructive nature. To earnestly call attention to an influence so adverse and dangerous to the public health, and to ask from the sanitary authorities that protection for the people which all good governments are bound to give, has been in so far the principal object of our discussion. While in the foregoing we have not presumed to

formulate any act or provision looking to the control of the sale and use of the dangerous caustic alkalies, we would suggest that the attention of the National Board of Health be called to the importance of securing from the general government some legislative precept or command by which manufacturers shall be compelled, in view of the perversion of their intent of the article as to soap-making, to have ineffaceably stamped upon the tin can containing the concentrated lye, and written indelibly on their labels the word "*poison*," and for the unlettered, that everywhere recognized warning against danger—the death-head and cross-bones—the black flag declaration of war against humanity.

Some equally effectual warning should be attached to any form of package in which such poisons are sold. Why should strychnine, arsenic, belladonna, and even laudanum, to be handled only by scientific druggists and physicians, be so marked, when this more frequently fatal agent, handled principally by the comparatively ignorant and uneducated, be cast upon the people with no caution, warning, or even hint of danger?

Treatment of Œsophageal Strictures after Chemical Injury.

Though in the history of the four cases forming the basis of this paper, the methods and devices of treatment by dilatation have been more or less particularly dwelt upon as the only course found necessary in what appeared to be several extreme cases, it is thought best to consider the comparative advantage and the dangers of some of the surgical operations that have been recently practised. These are internal œsophagotomy, œsophagostomy, and gastrostomy. Each one of these has been practised in cases wherein there was a real or supposed impossibility of pursuing dilatation to a successful result. In some of the cases in which these cutting operations have been performed, as in obstructions by tumors and cancerous affections of the tube, œsophagostomy or gastrostomy were the only resource, if any attempt at surgical relief was to be made. Such cases, however, do not come within the purview of our discussion, as not properly to be classed as strictures. Internal œsophagotomy,

then, as the only surgical operation claimed to be applicable in the class of strictures we are discussing, is the one which may here be considered. Dr. Morell Mackenzie[1] and Dr. J. O. Roe, of Rochester, N. Y., are the two surgeons who have more recently and prominently practised internal œsophagotomy in cases of stricture and other obstructions of the gullet, while the record of previous operations by others have been given by them. Dr. Roe[2] regards the operation as "one which must take its deserved place among the operations in the œsophagus," and the published statistics of the operation show that it has been performed from the time of Maisonneuve's three operations, 1861–'62, to his own two quite recently, some fifteen times. Dr. Mackenzie, though reporting one measurably successful operation of his own, yet discusses these, and the results of all internal œsophagotomies by others, in the most candid, impartial, and circumspect manner, and thus we are enabled to compare continued and progressive dilatation as presented in the four cases of the present paper with the result of some fifteen cases in which dilatation was attempted to be supplemented and facilitated by the division of the stricture with the œsophagotome. In his own case, that of a man, the incision was made in the mid line behind, dividing the stricture from below upwards. "There was no serious pain, but in a few hours the patient began to feel some discomfort over the base of the right lung, and unmistakable signs of pneumonia soon afterwards showed themselves. Having continued to manifest signs of pulmonary disease from the time of the operation, the patient died three months after, and at the post-mortem both lungs were found considerably congested, and presenting patches of pneumonia. The right pleural cavity contained a large quantity of sero-purulent fluid. Commencing about one inch below the cricoid cartilage, and extending downward for three inches, the walls of the œsophagus were found to be slightly thickened, hard, and uneven on the inner surface, the lumen of the gullet

[1] Dr. Mackenzie refers to two successful operations by Dr. E. C. Berg, of New York, in Arch. of Laryngol., Jan. 1883.

[2] See N. Y. Med. Record, Nov. 11, 1882.

being considerably restricted for that extent. At the lower part of the stricture an incision about one inch long was found extending through all the coats of the tube below, and through the mucous and part of the muscular tunic, for the upper half of the length, the wound showed but little signs of cicatrization." Dr. Mackenzie remarks on this case that "the pulmonary inflammation to which he ultimately succumbed came on so soon after the operation that it is most probable there was a causal relation between the two events."

The occurrence of structural and inflammatory changes in parts and tissues, distant from the seat of both the strictures and the operation has been quite a frequent sequence upon internal œsophagotomy, and in many of the cases under circumstances in which a perforation of the coats of either the gullet or the stomach could not be charged with this result. Besides others mentioned both by Dr. Mackenzie and Dr. Roe, two of Maisonneuve's three cases were found to have died of peritonitis, one of them died the eighth day after the operation. There was intense peritonitis, the origin and source of which, says Dr. Roe, were entirely unknown, though the cause seemed to be in the pelvis. In one of the Studsguard's cases, a girl eight years of age, who had swallowed lye and was operated on by internal œsophagotomy, the incision being made from above downward through a strong elastic obstruction, two hours after had some pain in the cardia and back, relieved by throwing up some clear blood. Her voice got thick and she could only speak with difficulty, and three times in the afternoon there was much oppression in the chest and dyspnœa, so much so that she grew bluish-red in the face, and it appeared as if she would suffocate. In the last case of Studsguard's we have functional disturbance in the lungs and stomach, which being distant from the seat of the stricture and the operation, could not have been caused by a perforation of the tunics of the gullet, and of which otherwise we have no evidence. Leaving out of our consideration for the present the more obvious (and momentous) dangers of the operation candidly admitted by Mackenzie and Roe, such as perforation of the wall of the gullet, resulting in fatal mediastinal

or pleural abscess, exhausting hemorrhage, and œsophagitis, I have grouped together the class of sequences as above seen, in order that they may be recognized as liabilities and dangers inseparably attaching to any internal œsophagotomy, according to the plainest and most unanswerable physiological reasoning and experiment. No one familiar with the experimental researches of John Reed and others upon the functions of the pneumogastric nerve can fail to find in these results the true interpretation and the probable cause of the phenomena referred to as the not infrequent results of section, or injury of the par vagum; pulmonary congestion, parenchymatous infiltration, pseudo-pneumonia, crepitant râles, dyspnœa, and a disturbed circulation in the structures supplied by the branches of the several trunks, are all familiar results of experimental section of the trunks of the pneumogastric.

As, from the beginning of the present paper, the pneumogastric has been recognized as a most important factor in the production of reflex tonic, and enduring spasm (*spasmus tonicus*) of the circular fibres, which the writer claims it is impossible to distinguish from organic stricture, and which produces the fatal result by starvation, he believes more frequently than the inodular kind. So we now wish to call attention to the fact that it is to the wounding or section of the trunk of the pneumogastric, or of some of its more important branches in internal œsophagotomy that are due the functional and structural changes in the vital parts to which it is distributed, and to call attention to this as one of the most momentous risks to be incurred in the operation as at present devised. We are instructed that the safest line and the only one that ought to be adopted for the incision is along the middle of the posterior wall of the gullet. Leaving out of view for the present the abundant blood supply along this wall, by reason of the anastomotic chain of the aortic œsophageal arteries, rendering incision here most liable to hemorrhage, as it has often been found, and also the tendency to future spasm which repeated wounding of the sensitive mucous lining will produce, let us recognize a danger here before not mentioned, but obviously as much or more to be dreaded even than hemor-

rhage: The trunk (*œsophageal plexus*) of the right pneumogastric nerve applies itself to the posterior wall of the gullet, at the arch of the aorta, and is conducted through the posterior mediastinum to the cardiac end of the stomach on that surface of the tube, while the left trunk of the pair pursues a similar course on the anterior surface of the gullet to the pyloric end of the stomach, where they both supply that organ and become tributary to the solar plexus of the organic system of nerves. In their course downward they each supply abundantly the lungs of their respective sides, while to the œsophagus multitudinous filaments are furnished to the muscular and mucous coats. In Dr. Mackenzie's operation, the incision having been made according to rule upon the posterior wall of the gullet, closely to which is attached the right trunk of the pneumogastric, it is a significant and apposite sequence that crepitant and moist râles, and dyspnœa with serious pulmonary disturbance, continuing for three months, corroborated by a post mortem showing pulmonary infiltration and pneumonic patches, should have followed the operation; all these pertaining almost exclusively to the right lung, or that supplied by the nerve, which, if cut experimentally, would have resulted in these same functional and histological changes. As to the peritonitis and other disturbances of the abdominal viscera, though in the opinion of the writer they can be legitimately attributed to the same cause, he will, after suggesting the explanation, leave it to the decision of others to adopt or reject the induction; but it will be remembered that the gastric branches of this pair are distributed likewise to the omentum, spleen, pancreas, liver, and gall-bladder, and that the pneumogastric is profoundly concerned in the circulation and functional activities of the abdominal viscera.[1]

Such then is the "causal relation between the two events" distinctly recognized, but not explained by Dr. Mackenzie; also applicable, we think, to the peritonitis in Maisonneuve's cases,

[1] In the Ophidian reptiles the pneumogastric takes the place of the organic system, and it has ever been recognized as a controller of vascular action in the organs to which it is distributed. Until recently it was known under the significant name of "the lesser sympathetic system of nerves."

viz., section or injury of the right pneumogastric nerve at the time of the operation.

Thus, it will be seen, in the mind of the writer at least, there is added another momentous danger to warn us against, if not to forbid entirely the operation of internal œsophagotomy, and leading hence to the acceptance of the carefully weighed estimate given by Dr. Mackenzie in regard to the three procedures of œsophagostomy, gastrostomy, and internal œsophagotomy. "From an examination of the results of the published cases, internal œsophagotomy does not appear to be a very satisfactory operation. Of the seventeen cases in which it has been practised, four died, *i. e.*, 23.5 per cent. This estimate includes only cases which proved fatal within fifteen days of the operation; the mortality would doubtless appear much higher if all the cases were counted in which death, though directly traceable to the operation, did not occur within the above-mentioned period." . . . . "As regards internal œsophagotomy, increased experience will probably show that, though its immediate results are not so frequently fatal, its ultimate effects, when successful, are less beneficial to the patient than those of either gastrostomy or œsophagostomy." To which may be added that, considering all its dangers to life, its doubtful permanent utility, and the encouraging results of dilatation, it is an operation which hereafter must depend upon either enterprise or desperation for its adoption, and upon only accident for its success.

Conclusions.

The following results may be summarized from the foregoing discussion of œsophageal strictures from chemical injury:—

1st. That in the definition and classification of stricture of the œsophagus, all obstructions to deglutition resulting from morbid growths or sarcoma, carcinoma, or from abscess or aneurism, bearing upon the walls of the tube, and diminishing or obliterating its cavity by extraneous pressure, should be eliminated, and that the term should be confined to narrowing from histological or functional changes occurring in the structures of the wall itself.

2d. Of stricture proper, as in other canals, the forms of organic and spasmodic exist; but in the œsophagus, on account of its muscularity, abundant supply of sensori-motor nerves, and its functional intent of reflex activity, spasmodic strictures are almost invariably found to attend and to complicate the cicatricial or organic form, and often to occlude the tube under abrasion and injury when organic strictures do not exist, in a degree of themselves to disable deglutition.

3d. That in cases of prolonged œsophageal disability, resulting in extreme emaciation and impending death, the fatal obstruction is the result more frequently of the spasmodic element, than of the inodular deposit imbedded in the walls of the tube, which narrow it, but seldom alone prevents fluid ingesta. It is the frequent and long enduring attacks of *spasmus tonicus*, lasting often from three to ten days, obstructing the passage of instruments and preventing nutrition, that is apt to mislead the practitioner, and by repetition compass the death of the patient.

4th. That a careful consideration of the anatomy and normal functional activities, as well as the habitudes and intent of the œsophagus, will corroborate the above views: for in its last analysis the normal act of deglutition is but an alternation of contractions and dilatations, by reflex excitation of muscular fibres.

5th. That the assumed analogy between urethral and œsophageal strictures, upon which the pathology and treatment of the latter have been based (Maisonneuve), fails in so many important particulars as to render the one a misleading and dangerous guide in the management of the other. In the one, spasmodic strictures are almost entirely confined to a particular and restricted region; while in the other, on account of its universal muscularity, its abundant nervous supply, and functional reflex activity, spasm in the circular fibre, of the most enduring and tetanic character, and indistinguishable from organic stricture, is apt to be excited in all portions of the canal, by irritation of the mucous lining.

6th. That these spasmodic ring-contractions are liable to be mistaken for, and incised as organic strictures, detrimentally

wounding the sensory filaments, thus increasing the number of points for the excitation of future tetanic constrictions.

7th. That the assumption of a guiding analogy between internal œsophagotomy and internal urethrotomy,[1] will at once be recognized as still more disastrous and dangerous, when the important anatomical differences, and consequent risk are to be considered. On the one hand, incisions in every part of the urethra, to any reasonable or even unreasonable extent, are not necessarily fatal, as both hemorrhage and extravasation can be controlled, or guided to a harmless result; while in the case of the gullet, surrounded by and in close contact with vital parts, inaccessible to styptics, and the only dependence for arrest of hemorrhage being spontaneous cessation, the fatality resulting from a penetration of its thin walls; and lastly, the section or wounding of the par vagum, almost unavoidable—all combine to demonstrate that in the plausible analogy there is no parity whatever, either in conditions or results.

8th. That in cases so desperate as to require a cutting operation, on account of failure in every possible method of nutrition, gastrostomy would be less dangerous and more permanently beneficial than internal œsophagotomy.

9th. That in view of the spasmodic nature of the affection, early progressive and long-continued dilatation is, par excellence, *the treatment* for œsophageal stricture resulting from chemical injury; that the dilators used should be smooth and flexible, conical in shape, and scaled as to size from filiform to that of the full diameter of the normal gullet.

10th. That dilatation may be begun as soon after the injury as inflammatory conditions will permit. The existence, or supposed existence of abrasions or ulcerations in the mucous membrane should not delay the beginning of dilatation. The contact of the dilator, instead of increasing, actually lessens the liability to tetanic spasm, by exhausting the reflex excitability of the morbidly sensitive membrane. Sedatives given hypodermically will often assist by relaxing the tonic rigidity of the circular fibres.

[1] It cannot be denied that this was the beginning of internal œsophagotomy with its originator, M. Maisonneuve.

11th. That the practice of dilatation should in each case be instituted and continued by the surgeon until fully established, after which it may in many cases be intrusted to the patient himself; or, if a child, to the intelligent parent or nurse, under the surgeon's supervision.

12th. The length of time during which dilatation should be practised cannot be limited to any particular period. After the deglutition of solids has become practicable, dilatation may be less frequent, as the passing of the alimentary bolus will adequately replace the action and intent of the bougie, in overcoming morbid sensibility and in restoring the natural reflexes of the tube.

13th. That during the treatment of dilatation, rectal alimentation will be found a valuable means of supporting the patient; and since it is known that life and health have been maintained during five years by milk, eggs, and meat pulps placed in the rectum, no case, even of complete and permanent closure of the gullet, could be justifiably subjected to the dangers of any cutting operation for the object of nutrition until after the failure of systematic rectal feeding as an adequate and permanent means of supporting life. This method is certainly no more unnatural and is less objectionable than that of introducing food into the stomach through an opening made by the operation, not devoid of danger, of gastrostomy. As a large majority of the cases of chemical injury to the gullet have been found to result from the careless sale and ignorant use of alkalies applied to domestic purposes, and as such sale and use are more liable to increase than to diminish in the future, the Legislature of each of the States and the national Congress should be petitioned, through their several sanitary organizations, to enact stringent laws requiring that all packages of such dangerous articles thus distributed among the people shall be plainly marked with the word "Poison," and for the warning of the unlettered that the death-head shall also be prominently emblazoned upon the can, box, wrapper, or other containing inclosure.

REPORT OF CASES ILLUSTRATING THE CONDITIONS OF LUXATION OF THE ULNA IN CONNECTION WITH COLLES'S FRACTURE.

By E. M. MOORE, M.D.,
OF ROCHESTER, NEW YORK.

[Read September 13, 1881.][1]

I do not propose, in the short paper I present you, to undertake a general treatment of the subjects connected with the fracture of the radius. The literature is voluminous. But I simply desire to exhibit some specimens of fracture at the lower end of the radius that have fallen under my observation. They are, however, all that I have collected, and of course present matters for inference.

I have written two papers which were read before the New York State Medical Society: one in February, 1870, at which time I announced some views which were not in accordance with those commonly received, and derived from the observation of a single case; and another, in 1880, reiterating these views, confirmed by additional cases that had come to notice during the intervening time. I will, therefore, quote from these papers freely, and bring to your notice the cases that have come into my possession since the last writing.

Before quoting the cases, I might premise that the doctrine I have taught my classes for the last ten years may be thus succinctly stated. The fall comes upon the palm of the hand, and the radius which sustains the full force of the strain gives way at a point near the wrist. The line of fracture is usually an oblique one, starting from a point on the anterior aspect of the radius, near the articulation, and running backward and upward,

[1] Received too late to appear in its proper place.—Editor.

making the posterior surface of the lower fragment longer than the anterior. It is seldom more than an inch from the joint, and seldom so far. But the lines of fracture are very various, sometimes simple and nearly transverse, but often comminuted to the last degree. The luxation of the ulna probably exists in more than half of the cases. I now feel quite sure, though I cannot demonstrate it by dissection, that a proper observation of the relation of the head of the ulna to the carpus will make a just diagnosis of its luxation or non-luxation. The force of the fall may be just balanced by the strength of the radius, but in the nature of such accidents this would not often be the case. In most, there would be some force still to be borne. Upon what structures does this fall? The radius instantly, on fracture, ceases to afford resistance, the hand is carried still further back, and then comes the strain on the attachments at the end of the ulna. It will be remembered that the ulna does not articulate with the carpus, but there is a distinct synovial cavity between its head and a strong membrane called the triangular fibro-cartilage. The membrane takes an origin from the rim and side of the radius, and, covering the head of the ulna, is inserted in the pit at the root of the styloid process. As the hand, with its broken fragment of the radius, is forced backward, the strain is often sufficient to rupture the connection between the two bones or to break the ulna near the head. I have seen this double fracture twice only. The rupture takes place at the weakest point, which is its insertion in the pit at the root of the the styloid process. But this is not the only resistance. The styloid is held to the carpus by the internal lateral ligament, which takes a very firm hold upon the end and radial surface of the styloid. This also gives way, and usually does so in a peculiar manner, viz., by pulling off the surface of the bone, which proves to be weaker than the ligament. Thus the remaining styloid is brought to an edge like a gouge-chisel, and is shortened about one-half. When these resisting forces are disposed of, the end of the ulna, now laid bare, is pressed against the posterior annular ligament, and is apt to become engaged upon it either by a fold, or, what is more likely, by splitting its

fibres and hooking upon it. If very great violence has been used in the production of these lesions, the head of the ulna will be driven forward through the annular ligament and skin, thus producing a compound luxation. These opinions were urged by me upon the profession, chiefly in consequence of the autopsy of Mary Tumey, but also from the very greatly improved results of treatment founded on these views. The case is as follows:—

In May, 1869, Mary Tumey, æt. forty-five, in a paroxysm of insanity, threw herself from the third-story window of the hospital, striking the ground with both hands, receiving also a blow on the spine opposite the second and third dorsal vertebræ, destroying life at once. The examination of the right arm was commenced by removal of the skin from over the wrist, leaving the fascia of the forearm undisturbed. Rotation of the hand did not produce crepitus. The projection of the wrist backward was that of an extreme case of this fracture, and the ulna was carried well downward and outward from the axis of the forearm. On raising the fascia, the lower fragment was seen to override the posterior surface of the upper one, or the shaft of the radius, and to be placed at right angles to its natural position, while the hand was in a state of extreme flexion in relation to it. There was not the slightest impaction. The absence of crepitus was explained by the fact that the rough surfaces were not in contact. The fracture was transverse, and the surface of the lower fragment, which was about half an inch in length, came in contact with the periosteal surface of the back of the upper fragment or shaft. The entanglement permitted rotation, but not crepitus.

The next step in the manipulation was to press the displaced fragment into its place. I was surprised to find a resistance which seemed like muscular action. This being manifestly impossible, the cause was again sought for by a repetition of the movement, when the same result was produced, giving an elastic rebound. The explanation of this rebound was found in the peculiar position of the ulna, its luxation and ligamentous entanglement. The cause of the retention now became manifest; for the styloid process was so far projected as to catch the fibres of the annular ligament, and the ulna being prevented from rising, forced the wrist back.

Various attempts at reduction were made at this stage of the examination.

The ordinary plan of bending the hand toward the ulnar border of the forearm, resulted in tightening the annular band on the styloid hook, and no pressure on the anterior surface of the ulna could cause its liberation; but, on the contrary, insured its retention. An incision through the annular ligament revealed the nature of the separation. The internal lateral ligament was torn away from its attachment to the styloid, by separating the scale of compact bony tissue composing its end and inner surface (the ligament proving stronger than the bone), thus leaving the styloid as a rough and ragged hook to hold the annular ligament. The attachment of the triangular fibro-cartilage to the styloid was also torn off, but the rent was through the cartilage, leaving a few tags at the pit. The complete severance of all ligamentous restraint permitted the ulna to bulge outward and downward; and as the hand is carried backward and upward, the styloid hook is moved forward and held at the pisiform bone by the annular ligament. The ulnar extensor was carried toward the radial side of the ulna. When the ulna became free from the annular ligament, its head was moved toward the radius, and through the fibro-cartilage, rested against the wrist-joint, thus holding the hand out at full length, keeping the fractured ends of the radius in apposition, and furnishing the best of all splints, an entire parallel bone, in place.

Thinking it possible that the condition found might be peculiar, I proceeded at once to a similar examination of the left wrist. The fracture was oblique in two directions. Commencing within a quarter of an inch of the wrist-joint on the palmar surface, the line ran back to three-fourths of an inch, inclining to the ulna. The position of the ulna was the same as on the right side. The triangular fibro-cartilage was torn out with similar tags, and the annular ligament was folded into a similar cord, causing a rebound when the lower fragment of the radius was carried down into its place.

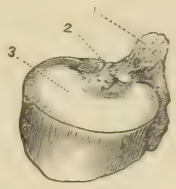
The only point in which the two differed was in the line of fracture, which seemed not to bear the least upon the question of reduction or retention.

The next case that came under my observation was a compound luxation.

John O'Hara, æt. 40, a laborer in a lumber yard, fell from a lumber pile eleven feet high, striking with his outstretched hand on

a plank road-way. The force was so great as to break the radius just above the wrist, also at its middle, and to dislocate it forward at the elbow. As, might be supposed, the radius was much shortened. The ulna was freed from its attachments, as has been described, and pushed through the skin. In this condition the case was presented to me. I removed the end of the ulna with a saw, dressed the fracture, and reduced the luxation at the elbow. The patient made a slow recovery, but finally obtained very good use of the arm. This bone I now present.

Fig. 1.



1. Styloid shortened by fracture. 2. Tags of triangular fibro-cartilage at the root of the styloid. 3. Smooth articulating surface at the end of the ulna.

It is absolutely as first found. This adds another, now the third case, where the triangular fibro-cartilage and lateral ligament were torn off in precisely the same manner; the two in Mary Tumey being only engaged in a rupture of the posterior annular ligament. But it will be remembered that the two in Tumey's case were only accompanied with the ordinary fracture of the radius at the lower end, and only one fracture in each arm. Where the luxation of the ulna was compound, the radius was shortened in an extraordinary degree by two fractures and a luxation at the elbow.

A case of this like O'Hara's has the full value of an autopsy, for the end of the ulna is in our hands, and the fracture can be diagnosticated without failure. I think, in view of the fact that the fractures in the case of Tumey were only of the ordinary character of those styled Colles's, with a good deal of the peculiar deformity, that the mode of luxation found here may be fairly concluded to occur from apparently slighter forces, but which are sufficient to break the bone. But I now present you a specimen which, in a singular way, leaves no chance for argument or cavil with reference to the ability of the apparently moderate force to produce the luxation I describe. I am indebted for this to my friend, Dr. Thos.

Collins, of Rochester, who was called to see a farmer, who, attempting to get out of his wagon, slipped from the hub, falling upon his hand. Upon examination, the radius was found to be broken, and the ulna was protruding through the skin. An attempt was made to reduce the luxation, but to his surprise and that of his counsel, Dr. J. F. Whitbeck, it was found impossible to reduce it. Therefore it was sawed off, and the fracture dressed. (I might add at this point that he made a good recovery, acquiring as good use of the arm as before the accident.)

I now present this specimen. Upon inspection, it will be seen that it presents a very extraordinary appearance. The shaft of the

Fig. 2.

Fig. 3.

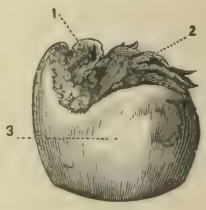


Fig. 2.—1. End of ulna, sawed surface. 2. Crust of eburnated bone.
Fig. 3.—1. Styloid short and smooth, rounded by absorption. 2. Tags of membrane attached to and obliterating articular surface. 3. Crust of eburnated bone.

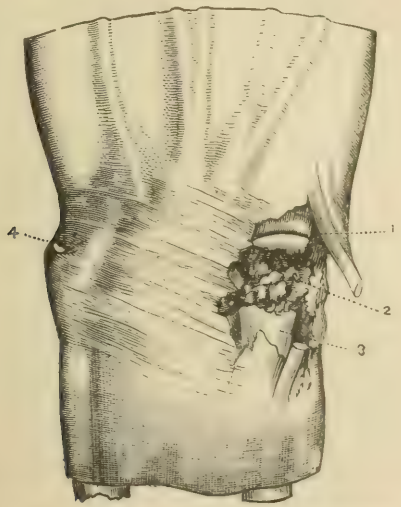
ulna is apparent, but the side and part of the end is covered with as fine a specimen of eburnation as it is possible to produce. The extremity of the ulna exhibits, instead of a smooth articulating surface, a rough one, made up of ruptured tags of cicatricial tissue. Inquiry revealed the fact of an antecedent fracture of the radius sixteen years before. The case, as rendered by Dr. Collins, is as follows: Jacob Winslow, æt. 75, received the injury as above described, October 25, 1873. In the year 1857, while walking in a saw-mill yard, he stepped on a slab and fell over on his hand, producing Colles's fracture. The mode of treatment at that time is unknown.

There can be but one inference from this case. We have, by the complete disappearance of the articulating surface, unmistakable proof of antecedent luxation. The eburnation also shows the effect of long and severe friction upon the side, the result of the severe and protracted labor of a farmer's life. The return of the luxation

was attempted, but in vain, in consequence of the extreme projection of the ulna, due to the antecedent accident. Several years elapsed without further proof derived from autopsy. But another case, resembling Mary Tumey's, occurred on the 11th of October, 1879.

Morris Huntington, æt. 56, a painter, fell from the roof of a three-story house, through a tree, to the side-walk. He was at once taken to the City Hospital, but lived only a few hours. A fracture of the wrist was observed, but he was so nearly moribund that it was not interfered with. I was invited, by the kindness of the staff, to make the autopsy. This was done by simply removing the skin from the posterior surface of the wrist, extending to the sides, thus coming at once upon the annular ligament. The end of the styloid chisel protruded through a rent in the ligament. This was carefully touched by Drs. Whitbeck, Montgomery, Little, Langworthy, Ely, Mallory, and others. It was precisely, in all its details, as before observed, and as I described it ten years since.

Fig. 4.



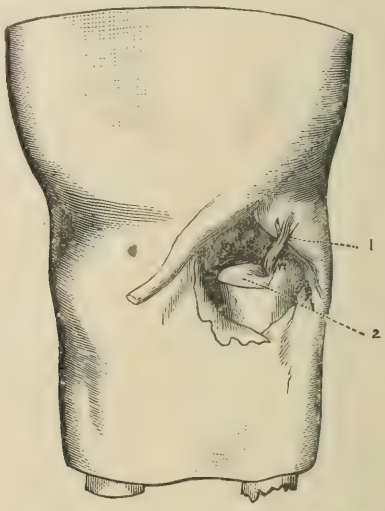
N. B.—In Figs. 4 and 5 the hand is uppermost.

Posterior aspect of Wrist.—1. Wrist-joint. 2. Comminuted fragments of the end of the radius. 3. Shaft of the radius. 4. Styloid process of ulna protruding through the annular ligament.

The next move in the dissection was the removal of the skin on the anterior aspect of the arm, and the division of the muscles and

tendons down to the bones. These were then lifted up just far enough to see the end of the ulna. The triangular fibro-cartilage was torn out at the pit of the styloid, and the internal lateral ligament torn off with the end of the styloid, as in the other cases. I

Fig. 5.



Anterior aspect of Wrist.—1. Tag of the triangular fibro-cartilage, unusually long. 2. Articulating surface of the head of the ulna.

now present you this specimen in its wet state. The rent and luxation can be seen, and the styloid chisel is almost precisely the same in shape as that of O'Hara's.

This case confirms all previous observations. It would not unlikely have been a compound luxation, but for the fact that the ulna was broken about three inches from the wrist, for the radius presents a condition of extraordinary comminution.

These five cases present all that I had seen up to this time, February, 1880.

In four of these cases the peculiar rupture of the styloid was identical. Two of them you have before you. The fifth has been modified by time. As they are all alike, I now feel sure of having described a luxation that has been overlooked. Luxations of the ulna have been noted, but they have been regarded

as irregular, and as having no definite character. But definiteness is now apparent. In the discussion upon this topic, at the meeting of the American Medical Association, held at Buffalo, I took the ground that for every compound luxation of any joint we will have a large number of simple ones. In the absence of statistics, which are not possible at present, I asseverated the round number of fifty. I repeat this statement on the ground that it could not be an exception to all our observations of other joints. It requires very great violence to produce a compound luxation at any place. Less force produces the simple luxation, and this we have very often with Colles's fracture.

Since the report of February, 1880, I have collected three cases from autopsy and one from operation, which I now present. These give variations from the singular uniformity previously observed in the mode of rupture of the ligaments.

Michael Harvey, entered the Hospital August 17, 1880, æt. fifty-seven, born in Ireland, occupation laborer. He fell from St. Louis Catholic Church, a distance of sixty feet, at 9 30 A. M. on the day of entrance. Patient was suffering from a compound Colles's fracture of the right arm, fracture of the spine at the fourth dorsal vertebra, with consequent paraplegia and shock. Did not rally, and died at 4 P. M. on the same day.

Autopsy.—The skin was raised from the back of the wrist and hand. The annular ligament was found punctured, the lateral ligament torn, without injury to the bone. The triangular fibro-cartilage also ruptured from the pit of the styloid. For the first time I found the internal lateral ligament torn in this manner, showing that the solution of continuity is due to a direct pull upon the ligament, and not a proper fracture, as was thought by Nélaton. The frequency of the removal of the surface of the styloid was remarked by Bouchet, as quoted by Malgaigne. In every other respect the luxation is as has been described in the other cases above.

Peter Moulin entered the Hospital December 26, 1880, æt. fifty-four, born in France, occupation locksmith. He was knocked from the Erie Railroad trestle-work at East Buffalo, and fell about twenty-five feet. Patient was suffering from Colles's fracture of right arm

and from fracture of base of skull. He died at 11.45 A. M., December 27. Autopsy at 12.30 same day.

The case of Peter Moulin gives information upon a point that I had been unable to decide. Every case I had before seen exhibited a rupture both of the internal lateral ligament and of the triangular fibro-cartilage. But in him we find the ligament entire and the cartilage torn. Of course there is a succession in time for the rupture of these torn ligaments, and, contrary to my expectation, the cartilage gives way first.

Frank Brown entered the Hospital January 15, 1881, æt. fifteen, born in Germany, occupation laborer. He was caught in a belt in Firmlich's sugar-works, Buffalo, and whipped around the shaft. Patient was suffering from fracture of left humerus, left radius, and ulna, upper third right femur, several ribs on left side, and Colles's fracture of left arm, epiphyseal. Died from shock in twelve hours. Autopsy on Colles's fracture 4 p. M., January 17.

This is the only autopsy of the epiphyseal fracture that I have ever seen. When first looked at it appeared to be a fracture without luxation. The lateral ligament was entire. But by dissecting up the tissues over the ulna carefully, between it and the radius the rupture of the triangular fibro-cartilage became apparent. Upon further inspection the styloid process is seen to be broken at its root, but not entirely separated.

I add one more case, still living.

Mrs. Ruth Burlingame, æt. fifty-five, broke the left wrist in January, 1880, by falling on the floor, producing the injury known to surgeons as Colles's fracture. The case was brought to my notice in June following, at which time I made an attempt to refracture it. But the union was too strong for any appliance that I had, and having failed, I resected the end of the ulna to shorten it, in correspondence with the radius. I now present you the bone.

It has been said that it has been long known that these luxations occur, but we wish information of the cases resulting from falling over on a level surface.

My reply is, that people seldom die from such falls, and we

cannot get autopsies of such. But here is one, the second of this kind.

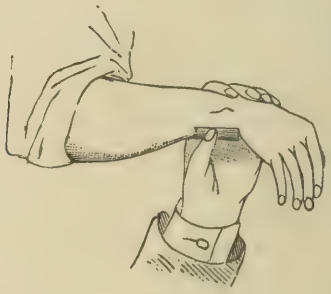
In a letter received from Mrs. Burlingame, a few days since, she says:—

"I can now use my arm as well as ever. If there is a motion, rotary or otherwise, that I do not have equally with my other arm, I am not conscious of it. I do not think of it at all. My arm is not very handsome, but so much improved in looks that I have nothing to complain of."

As the natural sequence of this diagnosis, illustrated by these numerous cases, I insist upon the great importance of the rectification of the luxation. Great pains must be taken to restore and retain the hand in position. Indeed I feel sure, when in place, a great variety of plans will be efficacious. But the one I have urged upon the notice of the profession has the merit of allowing a little motion at the wrist-joint during the treatment, and has proved useful in preventing the stiffness incidental to long-continued quiescence. I also believe that the weight of the hand will secure the best position of the head of the ulna. When there is the least relaxation of this weight, the contraction of the muscles will tend to shorten the arm, and this drives the ulna downward toward the pisiform bone. Moreover, the hand takes its natural semi-flexed position. I now feel a security of good results, such as I never before attained. I find in what I deem cases of luxation, a mobility of the end of the ulna incompatible with the integrity of the ligamentous apparatus. Every observer has recognized a difference of the form of the wrist in different cases. Some are curved backward—the genuine silver-fork shape—others have the hand carried more laterally. My convictions are, that those that present the wrist well curved back, are more apt to be those of luxation of the ulna with fracture, but the lateral bend implies generally shortening of the radius from fracture without luxation of the ulna. In my previous paper I proposed a form of traction and circumduction which is intended to disentangle the ulna and carry it up in its place between the tendons of the extensor

carpi ulnaris and extensor minimi digiti. I by no means wish to say that this is always necessary, for the great point should be not to relax any effort until complete restoration has taken place. The gentle handling that is ordinarily employed in the management of the fracture is out of place here. Great force is often necessary to restore the symmetry of the parts. We must keep in mind that a luxation is to be reduced, and should not cease our traction until the end is attained. Besides, the muscles will produce consecutive luxation instantly if the parts are not held firmly until the dressing is complete. With the thumb of the surgeon under the ulna, and the hand beneath,

Fig. 6.

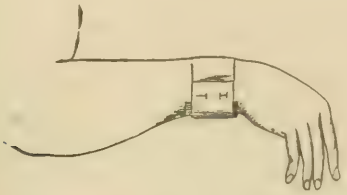


Showing position of Roller Compress.

and the fingers upon the back of the wrist holding with great firmness, I apply my dressing. This, it will be remembered, consists of a simple roller from half to three-quarters of an inch in diameter and two inches long. This is to be carefully placed under the ulna, abutting against the pisiform bone and slowly displacing the thumb. Then a strip of adhesive plaster of the same width is drawn with as much force as it will bear around the wrist, and pinned to prevent relaxation. The band of plastered cloth is carefully adjusted so that the distal edge is brought around on a line with the end of the radius. It is manifest that this bandage will grasp the broken fragment, and hold it to the end of the ulna. The rule, of loose dressing at first, is distinctly violated for a purpose. I repeat that I draw it as firmly as I

can, often breaking the plaster cloth. The dressing is entirely completed by the use of a sling, which must not be more than three inches wide. This must be placed over the roller, and is made of this width to cause the whole bearing to come on the

Fig. 7.



Roller with Adhesive Strip applied.

roller, which is both compress and splint. The hand is brought down and allowed to hang naturally. Thus its weight and that of the forearm is used to press the ulna upward into its proper place.

If all this is successfully accomplished, the broken fragments of the radius are easily kept in place.

The full length of the arm is maintained if the ulna does not fall down, and the tendons that run over the back of the wrist are so closely parallel as to make the best possible splint. I do not continue so gross a violation of the primary rule in dressing fractures as to retain the bandage in its tight condition; but, after six hours, cut it by thrusting one blade of a pair of scissors under it on the back of the wrist, dividing it completely. The few hours of such retention seem to be sufficient. The meagreness of the appliance has startled some who have attempted its use. But any addition that I have made has injured it. I find there is a strong disposition on the part of the patient to lift up the hand with the sound one. With unintelligent patients this is often troublesome, and a splint of iron, thin, such as hoop iron, bent so as to come over the back of the wrist and hand, well padded, and bound upon the forearm, but not upon the hand, will guard against this error. I am careful not to bind the hand to it, for I desire the constant action of gravity. The position

can be maintained, even in the recumbent posture. The slight motion in the joint, which will necessarily be produced by allowing it to hang freely, prevents the stiffness that is so often a very serious inconvenience after the treatment of this fracture.

Fig. 8.

Colles's Fracture; dressing complete.

There is another question to be answered: How good are the results? My experience has been considerable, as my professional brethren have shown me their cases when treated after my method. When there is much comminution there will be some absorption of the bone. This will produce a slight deflection. But if the fracture is transverse, deformity can hardly be said to exist. As in other luxations, the union of the ligaments is apt to result in their elongation, making, when the cure is perfected, a certain laxity and mobility of the head of the ulna after the fracture is perfectly firm. When the fracture is transverse, I have removed the bandage at the end of two weeks, but in other cases the treatment has been extended to four weeks.

I would remark that the reputation of the pistol-shaped splint,

Bond's, Levis's, and most others, can be explained. If great pains is taken to restore the parts, the flexed position of the hand will have a tendency to keep the restored ulna in place, especially if suitable compresses are used. These can be well adjusted in Bond's and Levis's splints. Moreover, in those cases in which there is no luxation, the results are apt to be good in the fixed position, for the full length is obtained because the ulna is not disturbed. Where luxation occurs, the muscles have a constant tendency to displace the ulnar head, which, when slipped downward in the least, is apt to remain. I therefore prefer gravity to any fixed splint; for the compresses, even if applied tightly, will yield, and any relaxation of the retentive force is apt to be accompanied with displacement. Moreover, by my simple dressing, the parts are under view without its derangement. I would urge the profession to banish the fear of its use as leading to disaster, with the full conviction, founded on the experience of my friends, that once understood and practised, no other would be thought of.

In view of the excellent results, where the end of the ulna was removed, I am convinced that the restoration of the wrist to a much greater degree of usefulness, in cases of long standing, can be obtained by a resection of the end of the ulna, so as to make the radius and ulna of the same length. When the radius is broken, producing the injury known as Colles's fracture, shortening of the bone is sure to take place, unless it is well drawn down and maintained in place. When there is comminution of the bone it is difficult to prevent this by any method. A shortened radius, with the ulna projecting forward and even pressing on the pisiform bone, often keeps the wrist in a painful condition for a long time. Moreover the motions are much impeded. The experience of Mrs. Burlingame affords a precedent for shortening the ulna to correspond with the shortened radius. A "flail" joint is almost as useful here as the normal one, and far better than the new one that nature creates near the pisiform bone. I never hesitate to refracture the radius when the result is bad. But, after six months have elapsed, the radius, if broken again, has suffered so much absorption that the restoration of

the ulna near its normal position cannot be maintained. The better plan is to resect the end of the ulna and not interfere with the radius. In one case of three years' duration, I broke off the radius at the place of original fracture and elevated the ulna with much benefit to the motion of the wrist and fingers, but with a rather increased deformity, the ulna projecting laterally.

INDEX.

Air-passages, foreign bodies in, 117
Artery, aneurism of posterior tibial, 9
——— femoral, traumatic aneurism of, 453
——— ligation of primitive iliac, 225
Astragalus, dislocations of, 265

Barton, J. M., 503
Bontecou, R. B., 505
Breast, condition of cicatrices after complete extirpation of, for carcinoma, 171
——— influence of operations in carcinoma of, 1
Briggs, W. T., 137

Cabell, James L., 23
Campbell, Henry F., 517
Conner, P. S., 285

Empyema, traumatic, use of the trephine in, 241

Femur, fracture of the neck of the, 313, 333

Gross, S. D., 253
Gross, Samuel W., 1, 171, 461
Growths, morbid, removal of, from the bladder, 505
———————, value of early operations in, 253
Gunn, Moses, 83

Hip-joint, bony anchylosis of, 111
Hutchison, Joseph C., 159

Inflammation, local processes of, 185

Jaws, treatment of closure of, 469

Kinloch, R. A., 9

Levis, R. J., 99

Marks, S., 307
Mears, J. Ewing, 173, 469
Moore, E. M., 111, 313, 551
Morbus coxarius, 159

Nancrede, C. B., 185
Nephrectomy for medullary carcinoma, 461
Neuralgia, trifacial, removal of Meckel's ganglion for relief of, 489
Norris, Basil, 265

Obturator, rectal, 509
Œsophagus, strictures of, 517
Ovariotomy, treatment of pedicle in, 173

Packard, John H., 19, 225
Patella, treatment of transverse fracture of, 99
Prince, David, 509

Richardson, T. G., 241, 453

Senn, N., 167, 333
Skull, treatment of fractures of, 83
Sternum, trephining of, 307
Surgery, sanitary conditions in, 23
Surgical cases, report of, 19

Tarsus, excisions of, 285

Ulna, luxation of, in connection with Colles's fracture, 551
Urethrotomy, external median perineal, 505

Vanderveer, A., 489

Watson, B. A., 205
Weist, J. R., 117
Wounds, antiseptic treatment of, 137
——— Lister's system of aseptic treatment of, 205
Wrist-joint, splint for treatment of synovitis of, 503

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